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FLYING-FISH FLIGHT, AND AN UNFIXED LAW OF NATURE

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THE controversy amongst naturalists as to whether flying-fish do or do not flap their wings in flight has become so one-sided as almost to represent extinction — as a controversy.

It is desirable, if possible, to revive it a little, by carrying the argument into new ground: first, because the one side which is at present believed in would appear to be the wrong one; and, secondly, because it seems to have escaped the notice of the other that this is capable of proof.

The arguments, if they may be so called, hitherto in use are simple assertion and denial, and may be summed up into:—

“Flying-fish do fly, moving their wings with extreme rapidity. I have carefully and frequently watched them and there can be no doubt whatever about it.”

And the converse:—

“Flying-fish do not flap their wings, but use them as aëroplanes, like swallows when in skimming or sailing flight. I have carefully and frequently watched them, and there can be no doubt whatever about it.”

Somewhat similar remarks will be heard in any ordinary group of ship's passengers watching the fish. Some will insist that they see the wings flapping, and some will aver that they are quite still.

But among scientists wing-flapping is undoubtedly very much the under dog and the carefully written paper by Captain Barrett-Hamilton (*Ann. Mag. Nat. Hist.*, ser. 7, vol. 11, p. 389, 1903), also a convinced aëroplanist, perhaps expresses current opinion as well as may be; and even Professor Whitman (*Amer. Naturalist*, vol. 14, p. 641, 1880), who insists that he has seen "distinctly the individual flaps of the large pectorals," adds that this flapping "may be continued for the whole or part of the flight, but it is generally discontinued after the first few rods, and the course continued by a pure skimming or sailing movement"—thus showing that he, too, believes in the possibility of the aëroplane flight.

Proof that such flight by any known species of flying-fish is a mechanical impossibility is the new ground which I propose to take up.

In order to make clear what the aëroplane theory is, I quote from the *Encyclopædia Britannica* (art. "Flying-fish") the "chief results of the inquiries" (*Die Bewegungen der Fliegenden Fische durch die Luft*, Leipzig, 1878) of one of its chief exponents, Professor K. Möbius. These results, which seem also to have formed the groundwork of many subsequent articles, are — with certain omissions on my part for brevity's sake — summed up as follows:—

"They are more frequently observed in rough weather, and in a disturbed sea than during calms; they dart out of the water. . . . and they rise without regard to the direction of the wind or waves. The fins are kept quietly distended without any motion, except an occasional vibration caused by the air, whenever the surface of the wing is parallel with the current of the wind. Their flight is rapid, but gradually decreasing in velocity, greatly exceeding that of a ship going ten miles an hour, and a distance of 500 feet. Generally it is longer when the fishes fly against, than with, or at an angle to, the wind. Any vertical or horizontal deviation from the straight course, when flying with or against the wind, is not caused at the will of the fish, but by currents of air. . . . in a rough sea, when flying against the course of the waves; they then frequently overtop each wave, being carried over it by the pressure of the disturbed air. They. . . . fall on board vessels. This never happens from the lee side, but during a breeze only, and from the weather side. During the night they frequently fly against the

weatherboard, where they are caught by the current of air and carried upwards to the height of 20 feet above the surface of the water, whilst under ordinary circumstances they keep close to it."

The above is fairly representative of the aëroplane theory. There are, however, several variants to it, the most notable being the addition by later writers of the use of the tail, both as a propeller in air, and also as an explanation of the loud buzzing sound always heard when the fish fly near or over a boat, and which is really made — it seems odd to have to write it — by the rapid whirring of the wings.

Of this whirring or flapping motion Professor Whitman writes: "It is so rapid that it is not easily recognized at any great distance until experience has sharpened the eye." Therein lies, I think, the cause of the birth of the aëroplane theory, though I must add that experience need not necessarily sharpen even good natural sight into being able to see the wing-motion. Knack or chance may come in in such matters. Some time ago, for instance, I was astonished, whilst testing the shooting of a shot-and-ball gun at the butts, to find that in certain lights I could plainly see the ball during its whole flight, whilst the attendant, whose daily business it was to test rifles and guns, and whose sight was far superior to mine, tried over and over again but could not pick it up. So have I seen many watch the whirring wings and declare them to be still.

It is commonly accepted that in matters of observation an affirmative evidence is superior to a negative one. In the special case under consideration, the value of the affirmative true flight evidence is very greatly increased by the fact that the aëroplane contradiction thereof must be in proof of a unique act in nature without a known parallel. Flying lizards and flying squirrels are perhaps the nearest, but in both cases the aëroplane is, I believe, greater by far compared with the weight borne, and — of more importance — the course is certainly far less and falling, not horizontal, or rising, as is that of the flying-fish.

Surely, therefore, it is not too much to ask from the aëroplanists either a reference to some mechanical parallel, or else absolutely overwhelming evidence in favor of the marvellous — a fair expression if no parallel be produced. We do not receive the

evidence, for, as before noted, it consists of a series of witnesses very fairly divided as to whether they can or cannot see the wing-movement, although scientific writers on the subject nearly all follow the latter. We do receive reference to certain parallels, and I shall endeavour to examine these with such lights as I can find. The parallels are, first, the "sailing" or skimming flight of birds (swallows being usually mentioned), and, secondly, parachutes.

For purposes of comparison in this examination, we will take a typical flying-fish. I have the wings of one, which flew on board a steamer on which I was traveling, before me as I write. Its weight was just over a pound, and it had a wing-area of 62 square inches, very liberally computed.

Let us consider the bird-flight first. Concerning this we have certain recognized facts to guide us, for which I refer readers to Professor E. J. Marey's work on *Animal Mechanism* (vol. 2, pp. 221-225, 1874).

We are specially concerned in his acceptance therein of the division of birds into two main classes, *viz.*, those largely given to "sailing" or still-wing flight (which class is found to be endowed with a large wing-surface), and those which confine themselves more to the "rowing" or wing-flapping flight (which, as a class, have short and narrow wings).

"If," says Professor Marey (*loc. cit.*, p. 221), "we compare together two rowing, or two sailing . . .," arranging as far as possible "to have no difference between them except that of size, we shall find a tolerably constant ratio between the weights of these birds and the surface of their wings." Tables are added of this ratio in various birds, as found by dividing the square root of their wing-surface in square centimeters by the cube root of their weight in grammes.

I will from these tables give this ratio for three of the sailing birds and for three of the rowing birds, including the two lowest ratios of the latter. I will add on my own account the ratio for the flying-fish, which is quite properly comparable with birds in this respect.

Name	Weight = p in grams	Surface of wings $= 2a$ in sq. cm.	Ratio = $\frac{\sqrt{2a}}{\sqrt{p}}$
<i>Falco palustris</i>	208.76	1188	5.810
<i>Falco subbuteo</i> (?)	509.62	1684	5.138
<i>Hirundo urbica</i> (House martin)	18.00	120	4.180
<i>Columnba vinacea</i>	112.00	292	3.545
<i>Saxicola ananthe</i>	56.05	125	2.922
<i>Perdix cinerea</i>	280.00	320	2.734
<i>Exocatetus</i> (Flying-fish)	453.59	400	2.603

Note the place of the flying-fish. It is quite in its proper position as a very low order of wing-flapper, requiring great wing-speed to sustain it in air. Note also the representative of the swallow tribe, weighing considerably under an ounce, in its proper place in the sailing class. The *Hirundo rustica*, or swallow proper, would doubtless hold a higher place still — our principal parallel, whose featherweight ought to have protected us from the comparison.

The figures should be convincing; I will not, therefore, comment more upon this, but proceed to another test, *viz.*, to find what size of wing a one-pound (453 grams) fish would require to raise it into the sailing class. No birds are dealt with by Marey of exactly one pound weight; I will therefore take the next above and the next below that weight.

The *Falco subbuteo* above shown has a weight of 509 grams and a wing-area of 1684 sq. cm., with ratio of 5.138, and the *Corvus cornix* has a weight of 374 grams and a wing-area of 1156 sq. cm., giving a ratio of 4.717.

Our one-pound flying-fish, to enable it to sail, would thus require a wing-area between three and four times greater than the 400 sq. cm., which it possesses. And, mark this, even then it would only sail as birds sail, in favorable winds and circumstances, falling and rising and using the "rowing" flight frequently, as may be necessary, not as our fishes go, "without regard to the direction of the wind," horizontally, and close to the water, and, according to aéroplanists, with ever still wings! Further, "concave bird-like surfaces afford from 3 to 7 times as much support as

planes." (*Encycl. Brit.*, art. "Aéronautes,— re flight.") It has been pointed out to me that it is extremely improbable that a flying fish's wings can assume this concave shape. If this be so, "from 9 to 28" may be substituted for "between three and four" times, above.

Need I go on? I am afraid so — superstitions, especially learned ones, die hard. So to the second parallel offered us, the parachute. The term implies the act of falling through the air, and not the horizontal or the rising motion with which we are dealing. Still, the word has been used in explanation of the fish's supposed deeds, and I will try to deal with it and at the same time keep clear of the pitfalls which will surround the effort.

Professor Möbius puts the speed of the flying-fish as "greatly exceeding that of a ship going 10 miles an hour." George Bennett (*Wanderings in New South Wales*, vol. 1, p. 31, 1834), much quoted, puts its extreme time in air at 30 seconds "by the watch," and its distance at 200 yards; this works out at rather over $13\frac{1}{2}$ miles an hour, extreme rate. It will, perhaps, give a sufficiently large margin to call the fish's average speed 15 miles an hour.

Now if wind and a body, either or both in motion, meet at a rate of 15 miles an hour directly against each other, the body having 1 square foot of surface, the pressure exerted thereon will be 1.107 lbs. That, I think, implies that if a flying-fish weighing a little over a pound and having a wing-surface of 144 square inches (an impossibly large one, of course, for such a fish) were falling through still air, it would descend at the rate of about 15 miles an hour; or, on the other hand, if it were in a wind blowing 15 miles an hour straight upward from the sea (an impossibly favoring wind, of course) it would just be supported. I will leave it entirely to my readers to imagine the effect in the second case upon our fish of reducing its wing-area from the suppositious 144 sq. inches to its actual 62 sq. inches.

If the reader's imagination is not sufficient to drop the fish into the sea at once by the reduction, then let him add the effect of removing as much support as would be taken away by changing the impossible upward-blown wind into the ordinary horizontal one at the same 15 miles an hour speed, meeting the wings at an acute angle. There are pitfalls here, so I will avoid angles and

calculations, and merely point out that, however much scientists may differ as to the amount of the loss of the supporting power involved, none will dispute that there will be a very great loss.

Yet again, if these descents from favoring suppositions to sober facts will not convince, I must advance one more argument. It is, I believe, like the others, new ground, and I will give it a fresh paragraph.

Flying-fish, at the end of their first flight of usually about 10 to 50 yards, have a habit, especially when approaching the crest of a wave, of momentarily checking their wing-movement and slowing down from the blur of great rapidity into a pace in which the flapping of the wing becomes easily visible. This period of visibility is supposed by aéroplanists to be the only portion of the flight during which the wings move, and they even deny them at this time any supporting power whatever. It is their "period of occasional vibration" or "fluttering," and their explanation thereof will make a mechanician smile or feel sad, according to his temperament. I have already quoted it from Möbius, and it amounts to the wings trailing in the wind like a loosely flapping flag, thus not only depriving the heavy fish of the so called support of its miniature aéroplanes, but actually converting them into an active drag.

And yet, according to the theorists, at an extreme suggested speed of $13\frac{1}{2}$ miles an hour, the fish still sails!

Such an upsetting of one of the best known of nature's laws as all the foregoing implies would be impossible of final acceptance, even if we could not, as many of us can, see the flying-fish flying.

I studied the "vibration" or flutter periods very carefully this spring when returning from the Gulf of Mexico. Their object and method seemed simple and clear, and to be as follows: the slowing down from extreme wing-speed into visibility heralds an immediate increased effort of flight, often, if not usually, to enable the fish to surmount a wave. The fish is, in fact, pulling itself together for a spurt. The flutter, as was to be expected, is accompanied by a slight fall of the fish of perhaps 2 or 3 inches; but the spurt, at once put on, regains the lost elevation and lifts the fish well over the obstacle. This sudden rise of the fish (the "frequently overtop each wave" of Möbius) is constantly to be seen, and to many the wings seem still at this time.

The difference in the rates of speed of wing-flapping on different days is very marked. At times, and often for many successive days, it is noticeable that, although the bodies of the fish as they rise from under the steamer's bows are clearly and sharply defined their supporting wings have a peculiar hazy and blurred look, with a want of definition of outline which cannot be accounted for, for they seem to be still. Then a day will come when the fish, still fleeing in front of the ship, will move their wings less rapidly and their motion will become plainly visible. There are still many lookers-on who cannot pick it up, but for the rest the aéroplane theory is exploded for ever, and when next the swifter-moving wings are seen with the eye of knowledge the wonder is that there had been any difficulty. The haze and blur are exactly what should have been looked for under the circumstances.

We have all of us watched sea-gulls soaring quietly in a certain direction, but obliged to flap when they turn away, the vigor of the flapping varying more or less regularly with the direction in which they meet the wind. It is more than probable that the change of wing-speed of the fish varies for similar reasons in degree of rapidity, soaring being, as I have endeavored to show, quite out of the question. From whatever cause, it certainly does so vary.

A curious thing about the "vibration" periods is that they seem to offer fleeting glimpses of a satisfactory wing; for a moment, now and again, the wings have outlines and edges, and will also occasionally return a sun-glare to the eye from their wet glassy surfaces, such as might be expected from them when not whirring. Such a glare is also now and then momentarily to be seen when a fish ceases flying, and just before it strikes the water, if it be in the proper position with regard to the sun. There would, of course, be many long periods of this glare were the wings really still.

One or two more prominent fallacies are handed on from writer to writer, and often accepted as facts. One is that the fish are helped in their flight by the distention of their air-bladder. If such had any appreciable effect it would be that of impeding the flight, for the contained air being under compression would be denser and therefore heavier than the outside air, and the increased

size of the fish would merely check its speed as a hollow bullet is checked.

Steering-power is also denied to the fish by most naturalists. It is, nevertheless, a matter of common seafaring knowledge that they turn with deliberate intention. I have myself watched one fly towards the ship, and, circling back, finish its flight in a direction straight away from the ship. It approached within a yard or so of the side, close under where I was standing. The check of speed on its first taking alarm was marked, and during the turn of half a circle of about 10 or 12 feet radius which it made it could not have been flying at a rate of more than three or four miles an hour.

Again, they rise quite at will, though this power also is denied by aeroplanists. With reference to this, as well as to their power of steering, the late Earl of Pembroke, or Doctor G. H. Kingsley, joint authors of *South-Sea Bubbles*, says (p. 64, 7th ed., 1895): "Flying-fish *do* fly, moving their pectoral fins with extreme rapidity, moreover, they raise and lower themselves over the tops of the waves, and do *not* dip into them, . . . I remember between Panama and Rapa I used to see the cabin's bulls' eyes surrounded by a circle of scales every morning left there by flying-fish." They were making for the light. No ingenuity can fasten this upon "currents of air," which are credited with so many other impossible feats on behalf of these fish. This habit of theirs is quite well known, and is effected by raising themselves and steering, pure and simple.

Their taking a baited hook is also denied. As a matter of fact, a baited hook is the first part of the fishing-process of the Barbados flying-fishing fleet, with which I have been out. We had a blank day; but, according to the animated description of the boatmen, the struggles of the first victim bring round it swarms of sympathizers (as gulls flock round a wounded companion), and these are "raked" into the boat by the hand hoop-net, an enlarged edition of a round shallow shrimp-net without any handle.

I have throughout this paper spoken of flying-fish generally, for the wing-areas of all of the known kinds are to their weights and speeds such that the impossibility of their practical use as aeroplanes differs only in degree.

Flying-fish put on different aspects according to the state of air and sea. One is rather startled at times by the changes in their methods. In oily equatorial calms, I have watched them in numbers flying long distances with their tails in the water and their heads and wings in the air, the body making an angle of perhaps 30° or 40° with the horizon. The wake left in the water by the dragging tail showed, as well as I could judge, no signs of its having been used for purposes of propulsion, even in its own element, and it is, perhaps, simply to relieve the fish of its weight that it is so supported when there is no fear of the wings being caught by ruffled water; nevertheless the peculiar long lower half of these tails specially adapts them for use as auxiliary propellers to a fish which, with their exception is "a fish out of water"; and it looks so like a case of natural evolution, that I feel inclined to doubt the justice of my personal observation as to their non-use.

It would seem, from this habit, reasonable to suppose that the fish have the power of flapping their wings at various angles, as have birds, as ordinarily their bodies are fairly horizontal as they fly.

The flight of these fish is often described as "graceful," "light," and so on. To him who believes that they soar along easily for 200 yards without further effort than a preliminary leap from the sea, such an opinion may be a natural one.

To him who recognizes that such a leap is mechanically impossible, whether or not assisted by a continuous tail-movement, or to him, who, without thinking particularly about it, simply sees the heavy laboring of the wings as the fish patiently whirrs along its even, uneventful way, "graceful" and "light" are terms misplaced. Strenuous, persistent, plodding effort is the impression left upon the mind, the least failure in which effort means plumping into the water. One often sees this happen obviously without intention on the fish's part.

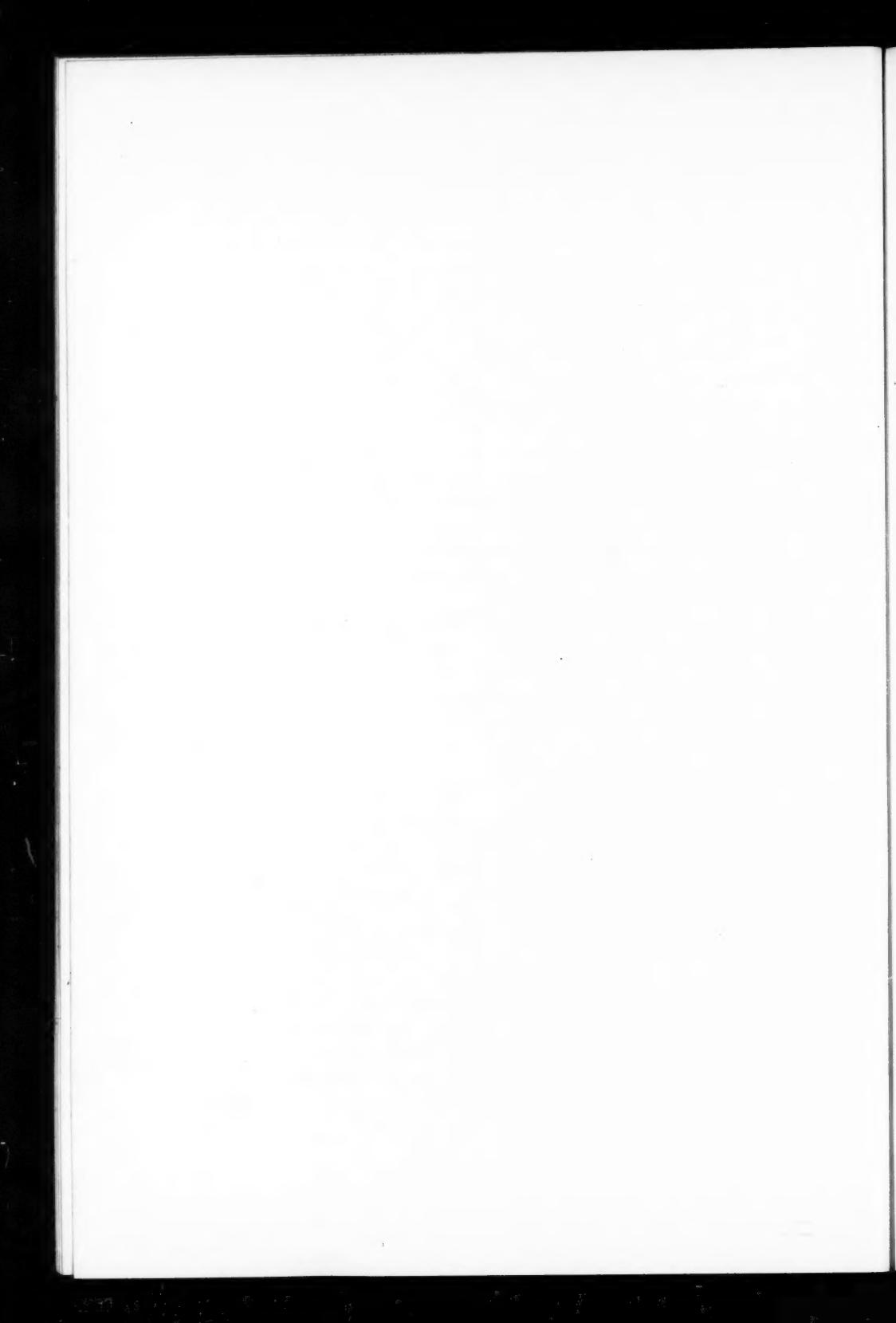
In conclusion, it is, I think, made clear:—

1. That flying-fish would require to have a wing-area several, and probably many times greater, according to their weights, than they actually possess to enable them to accomplish sailing flight in even such a restricted form as that carried out by sailing birds.

2. That we know of no parallel case in nature which would justify the assumption that the possession by these fishes of even such increased wing-area would of necessity enable them to sail long distances —(a) horizontally, or (b) close to an obstruction (the sea), or (c) in defiance of the direction of the wind; much less all three (a), (b), and (c) combined, as they commonly fly.

3. That their common flight is exactly what is to be expected of flyers holding, as they do, a very low wing to weight ratio — flyers capable of and of necessity employing, extreme wing-speed.

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DOUBLE HENS' EGGS

G. H. PARKER

THE presence of an additional yolk or of a second more or less perfect egg in a hen's egg, though not an unusual occurrence, is rare enough to excite the attention of those interested in natural phenomena and has been a matter of record since the time of Aristotle. A recapitulation of the early instances of this kind has already been given by Davaine ('61), who has also added much to our knowledge of double eggs. The following account contains a description of five such eggs which have come to the writer's notice in the past few years and present certain features worthy of record.

Of these eggs the first to be described was laid 26 June, 1905, by a hen belonging to Mrs. Prince Stuart of Wood's Hole, Mass. I am indebted to Mr. A. S. Pearse for the opportunity of examining it. The egg was unusually large, its major axis measuring 74 mm., its minor 55 mm. In form it was not unlike a normal egg except that the point was less certainly distinguishable from the butt than is commonly the case. The shell was almost white; near the poles its surface was smooth, but about its equator there was a broad band of unusual roughness. Within was a normal shell membrane inclosing a single mass of albumen containing two yolks. These lay one toward the butt, the other toward the point of the egg. The one toward the butt was approximately spheroidal with its major axis at right angles to that of the whole egg. It measured 34 mm. by 30 mm. The yolk nearer the point was smaller than the other one, by which it was indented on the side away from the point. It measured 21 mm. by 27 mm., and its major axis was also at right angles to that of the whole egg.

Though the two yolks were in intimate contact along their applied faces, they were organically distinct, since each possessed an independent vitelline membrane. So far as could be judged, they were of the same age, in that both had the appearance of freshly laid yolks.

The second egg to be described is one that I had the privilege of examining through the kindness of Mr. C. C. Spratt. It had been laid in the spring several years ago by a hen belonging to Mrs. C. H. Gould of North Bridgton, Me. The outer shell, which was thick but otherwise normal, was much broken; its two axes measured 54 mm. and approximately 73 mm. It was lined with a shell membrane and its contents were lost except for a small complete egg which it contained. This measured 33 mm. by 39 mm., and, though rather roundish in outline, it presented a butt and a point. Its shell was thinner than usual and its whole outer surface was granular. The inside of this shell was lined with a shell membrane and contained dried albumen and a dried yolk.

The three remaining eggs were laid by a hen belonging to Mr. F. Nielson of Medford, Mass. They were laid in March, 1903, and shortly after the laying of the largest one the hen died. When the eggs came to my hands, each had a small opening at one end. I am therefore unable to give their exact length but in other respects they were in excellent condition for examination. The smallest measured 43 mm. by approximately 57 mm., the next 48 mm. by approximately 56 mm., and the largest and last to be laid 55 mm. by approximately 71 mm. In each instance a point and a butt could be distinguished and the shells were of normal texture, color, and thickness. Each shell contained a shell membrane and a mass of albumen in which was imbedded a second smaller egg.

These eggs were used for exhibition purposes, but I was allowed to cut open the one of intermediate size, and the appearance of its section face is given in the accompanying figure. It will be seen at once that the inclosed egg is relatively large; it measured 45 mm. by 29 mm. A butt and a point could be easily distinguished on it. The chief axis of the small egg was parallel to that of the large one and its point and butt were just within the corresponding

parts of the inclosing shell. The shell of the small egg was rather thin; it was lined with a shell membrane and contained albumen which had withdrawn slightly from the shell wall, probably through shrinkage. Between the inner shell and the membrane lining the outer shell, was a mass of albumen, which was slightly discolored around the equator of the smaller egg and near its butt by a small amount of yolk substance. Aside from this neither the larger nor the smaller egg contained any evidence of yolk.

The internal condition of the other two eggs belonging to this set could not be ascertained, for the owner preferred to keep them in their present form. Judging, however, from what could be seen through the small holes in their ends, they contained relatively large eggs with firm limy shells like that seen in the egg that was cut.

An examination of the five eggs thus far described and a comparison of their conditions with those of other recorded cases of double eggs, have led me to the conclusion that at least two factors are concerned in the production of such eggs. Double-yolk eggs like the first one described, are due in my opinion to the simultaneous or almost simultaneous discharge of *two* yolks from the ovary instead of *one*, these two being enveloped by albumen, shell membrane, and shell in an essentially normal manner. Inclosed eggs on the other hand may be the product of an entirely normal ovary and may result from the abnormal action of the oviduct, in that a yolk normally supplied by the ovary may be abnormally covered, retained, and inclosed in another egg. Thus two factors in the production of double eggs may be distinguished: ovarian and oviducal.

That these two factors are really independent is indicated in several ways. First, they seem to come into play at somewhat

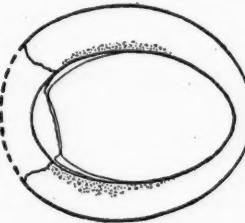


FIG. 1.—A double egg cut in longitudinal section from butt to point. The outer shell measured 48 mm. by about 56 mm.; the inner, 29 mm. by 45 mm.; both contained albumen, that of the inclosing egg showing yolk substance (drawn as granules in the figure). Beginning at the butt of the egg, the dotted line represents the reconstructed shell; the first solid line, the shell of the inclosed egg; the second, its shell membrane; and the third, the surface of its albumen. The spaces between shell and membrane, and membrane and albumen, are probably due to shrinkage.

different seasons. The double-yolk egg described in this paper was laid in June, and, though Bauer ('98, p. 304) and Immermann ('99, p. 7) record cases of this kind in December and Panum ('60, p. 186) in January, the great majority of such occur during the warmer part of the year, from May to August according to Immermann ('99, p. 7) or from March to September according to Panum ('60, p. 186). The inclosed eggs on the contrary are produced in the winter and spring. Thus the second egg described in this paper was laid at some time in the spring and the remaining three in March. A compilation of the published records of this kind shows the period to extend from December to March or April.¹ It therefore appears that while double-yolk eggs may be laid at any time of year, they are most abundant in summer and that inclosed eggs, so far as the records go, are limited exclusively to the winter and spring.

Another point in evidence of the independence of the ovarian and oviducal factors is seen in the condition of the hen. The laying of eggs with two yolks may become, as Landois ('78, p. 24) declares, almost habitual with certain hens. Bartels ('95, p. 143) states that the hen that laid the double egg described by him had often laid such eggs and Immermann ('99, p. 8) records the case of a hen that laid such an egg about every eight days. Apparently this is as much an organic peculiarity of certain hens as is the production of twins by certain individuals in the human species, and, while it may be called abnormal in that it is unusual, it is in no sense indicative of serious organic derangement or disease. The laying of inclosed eggs, however, is often followed by serious consequences to the hen. Thus the hen belonging to Mr. Nielson died shortly after laying the last of the lot of three inclosed eggs described in this paper, and the same fate immediately overtook the hen that laid the two inclosed eggs described by Fritsch ('95). Evidently the laying of such eggs indicates a more serious state of affairs so far as the hen is concerned than the laying of double-yolk eggs and brings out again a difference between the ovarian and the oviducal factors.

¹ Inclosed eggs have been recorded as laid in winter (Chobaut, '97), December (Philippi, '93), January (Parona e Grassi, '77), March (Collin, '94; Féré, '02), and at Easter (Schumacher, '96).

As a result of the action of these two factors, three classes of double eggs can be distinguished: first, those whose yolks have come from an abnormal ovary but have passed through a normal oviduct; secondly, those whose yolks have come from a normal ovary but have passed through an abnormal oviduct; and finally, those produced by an ovary and oviduct both of which have been abnormal in their action.

Of the first class little need be said. Although eggs with three yolks are extremely rare, those with two are of rather common occurrence and, as has been pointed out, they are often repeatedly laid by a given hen without injury to herself. As Immermann ('99, p. 10) rightly observed, these eggs fall into two subclasses: the first includes eggs in which the yolks have separate vitelline membranes, and the second those in which the two yolks are within one membrane. In the former the yolks were probably discharged simultaneously from separate ovarian follicles; in the latter both yolks very likely came from the same follicle. When these eggs are incubated, the two embryos begin their development together and proceed at about the same rate. In this respect they are in strong contrast with most inclosed eggs in which, as in the egg described by Féré (:02, p. 349), the inclosed yolk is in advance of the inclosing one in development. The two yolks of double-yolk eggs are usually each of normal size and in consequence induce the formation of a large egg, though the volume of the whole is usually not more than once and a half to once and three quarters that of a normal egg. The fact that hens can lay such large eggs repeatedly and yet without injury to themselves, shows that the death of the hen, which often follows the laying of inclosed eggs, cannot be attributed merely to mechanical causes.

Under this first class of abnormalities have also been placed eggs with yolks of unusual form, such as the apparently double-yolk egg described by Möbius ('95). Since such apparently double or partly double yolks often arise from a rupture of the vitelline membrane and a flowing out of yolk substance, they cannot of course be regarded as real examples of double yolks. Such yolk hernias may be due either to a weak vitelline membrane or, as Davaine ('61, p. 256) has suggested, to a constricted oviduct, but in either case they are not to be classed with true double eggs.

The second class of abnormal eggs includes those in which a normal yolk is received by an abnormal oviduct and in consequence becomes covered with an abnormal set of envelopes. This is represented by eggs that are normal as to contents, form, and size, but are contained in other larger eggs. Instances of this kind have been described by Barnes ('63; '85), Fritsch ('95), Chobaut ('97), and Gruvel (:01). Very likely the second egg described by Supino ('97) belongs to this class, but the inclosed egg, though of normal size, is said to contain an unusual yolk, indicating possibly an ovarian abnormality. Here also should probably be placed an egg recorded by Fére (:02) in which the inclosed yolk, though apparently normal, is contained in a small amount of albumen and what appears to be a thick egg membrane, but is without a shell.

In most of the instances just cited, the enveloping eggs are of two kinds. The first consists of shell, membrane, and albumen as in the cases described by Fritsch, Chobaut, and Gruvel; and the second possesses a yolk in addition to these parts, as in the eggs recorded by Barnes and Supino. The exact method by which a normal egg becomes inclosed in a second more or less complete egg is not wholly clear; but a discussion of this question will be deferred till the third class of eggs has been described.

In the third class of double eggs there is evidence of both ovarian and oviducal abnormalities. In examples of this kind the inclosed eggs usually consist of shell and membrane containing a mass of albumen and a small yolk, as in the cases recorded by Vaillant ('75), Parona e Grassi ('77), de Man ('78), Philippi ('93), Schumacher ('96), Herrick ('99a; '99b, p. 409), and Kunstler et Brascassat (:01); or inclosing albumen but without a yolk, as in the first egg described by Supino ('97), and those described by Herrick ('99b, p. 410), and Gruvel (:02). In this class two types of inclosing eggs might be expected: one with a yolk and one without a yolk, but, strange to say, of the nine instances¹ in which the descriptions are sufficiently full to allow this point to be ascertained, the inclosing egg always consisted of shell, albumen, and yolk.

¹Parona e Grassi ('77), de Man ('78), Philippi ('93), Schumacher ('96), Supino, two eggs ('97), Herrick, two eggs ('99 a; '99 b), and Kunstler et Brascassat (:02).

Notwithstanding the fragmentary character of the second egg described in this paper, it undoubtedly falls under the third class; and I am also of the opinion that the three eggs from Mr. Nielson's hen likewise belong here. It will be remembered, however, that of these three eggs the one that was opened presented the remarkable feature, not hitherto recorded to my knowledge, of the absence of yolks from both the inclosed and the inclosing egg, though traces of yolk substance were found in the latter. These traces lead me to believe that the inclosing egg originally contained a yolk which, however, probably broke and almost entirely ran out before the membrane and shell of this egg were formed.

To explain how such inclosed eggs reach their positions, at least two hypotheses have been put forward. According to the first of these, which has been advocated by Davaine ('61, p. 238), Schumacher ('96, p. 368), Herrick ('99b, p. 413), and Kunstler et Brascassat (:01; :02), an egg after having passed by peristalsis to the distal end of the oviduct and after having received its usual coverings of albumen, shell membrane, and shell, is supposed to be carried by antiperistalsis up the oviduct where it meets a second egg, and passing down with this, becomes covered by a second shell, and is laid.

According to the second hypothesis, which has been advanced by Panum ('60, p. 185), Chobaut ('97), and Rabaud (:02), antiperistalsis plays no part in the formation of inclosed eggs, but the egg which is to be inclosed remains in the distal part of the oviduct instead of being laid and is there overtaken by a second egg while the second one is still without shell. After the second egg has enveloped the first, a shell inclosing both is laid down.

In testing these two hypotheses, the chief question is whether or not there is any evidence for antiperistalsis. The common occurrence of a small egg with a limy shell in the albumen of a large one whose shell membrane is intact, seems to me inexplicable except on the assumption of antiperistalsis. Such an egg as the smaller one could not receive its shell except by resting some time in the distal part of the oviduct and it could not come to lie in the albumen of another egg whose shell membrane was not ruptured except by passing to a region in the oviduct above that in which the shell membrane is formed; as this region is proximal to the

shell-forming portion of the duct the operation seems to me to necessitate antiperistalsis. It might be assumed that the inclosed egg made its way into the albumen of the inclosing one by rupturing the shell membrane of the latter just as that egg reached the shell chamber of the oviduct. But there is no evidence in such eggs of a ruptured membrane as this hypothesis would require and indeed there is a case on record (Gruvel, '02, p. 73) in which the inclosed egg apparently met the inclosing one when the shell membranes of the latter were forming and, instead of rupturing them, the inclosed egg remained between the inner and outer membrane and never entered the albumen of the inclosing egg at all. I therefore do not believe that the inclosed egg enters the albumen by rupturing egg membranes but by meeting the inclosing egg by antiperistalsis high in the oviduct and before the membranes have been formed.

Another fact that seems to me impossible of explanation except on the assumption of antiperistalsis is the occurrence of "soft-shelled" eggs in the body-cavities of fowls. This has been recorded by Davaine ('61, p. 241) and more recently by Landois ('99, p. 52), who states that in one instance he found four such eggs in the body-cavity of a hen. Two of these were broken, but two were whole and had all the appearances of normal eggs except that they lacked shells. As there is no source for the albumen and shell membranes of these eggs except the middle and lower part of the oviduct and no way into the body-cavity except by the infundibulum, I believe the conclusion inevitable that these eggs, after the formation of their shell membranes, were moved proximally by antiperistalsis.

How antiperistalsis is excited in the oviducts is not understood. It has been suggested that an egg of small volume might induce such a movement and thus be returned to the upper part of the oviduct, but, though this cannot be denied, it must be remembered that, as Landois ('95, p. 32) has shown, small eggs such as are often found in large ones, may be laid by hens. Moreover, as was stated in describing inclosed eggs of the second class, eggs of normal size are often found within the shell membranes of excessively large eggs and must therefore have moved up the oviduct. Hence the small size of an egg cannot be the only cause of antiperistalsis, if in fact it is at all effective in this respect.

Although in the present state of our knowledge it is impossible to assign a cause for the setting-in of antiperistalsis, it seems to be a process that may occur not only once but even twice in the enveloping of a yolk. Davaine ('61, p. 237) quotes a case in which an egg presented the very unusual condition of three separate envelopes instead of two; these were a firm outer shell, a strong shell membrane, and a thin shell membrane, and a second case is given by Landois ('92, p. 34) of an egg of the red-backed shrike (*Lanius collurio* L.) that had three shells one within the other.

So far as I know, the only objection that has been raised against antiperistalsis of the oviduct has come from Rabaud (:02, p. 201), who claims that this process could not bring two eggs *together*, but would simply move them up or down the oviduct. But such an objection is formal rather than otherwise, for it must be evident to anyone who has watched the process as it occurs in the intestines that its local character is such that eggs could easily be made to approach in the oviduct through its means. I therefore dismiss this objection as without weight.

Although I believe antiperistalsis to be an all important factor in the formation of inclosed eggs, it is in all probability sometimes greatly restricted, as is indicated by an egg described by De Toni ('90) and the second of the two described by Supino ('97). In both these eggs the outer surface of the inclosed shell is adherent to the inner surface of the inclosing one; in other words, the albuminous investment of the inclosed egg is imperfect, as though the smaller egg, while resting in the shell chamber of the oviduct, was imperfectly enveloped by the inclosing egg, whose shell on forming adhered to the inner shell where the envelope was incomplete. Thus an inclosed egg might be imagined to arise after the manner suggested by the second hypothesis already given. But in both these instances the inclosed egg lies in the albumen of the inclosing one and within its shell membrane, and, as already pointed out, it is impossible to explain this position except on the assumption of at least some slight antiperistalsis. I therefore believe that the formation of inclosed eggs cannot take place simply by the undue retention of one egg till it is overtaken by another, as stated in the second hypothesis, but that in all instances some antiperistalsis occurs.

Admitting antiperistalsis to be essential to the formation of inclosed eggs, it is interesting to observe that, notwithstanding the migrations of these eggs, they appear to retain always the same axial relations to the oviduct that they had in the beginning. This is clearly seen in the eggs described by Barnes ('63), Chobaut ('97), Herrick ('99b, p. 410), Kunstler et Brascassat (:02), and Grivel (:02), as well as in one of those described in this paper, in all of which the point of the inclosed egg is in the direction of that of the inclosing one and not toward its butt, showing that the smaller egg has retained its original axial relations to the oviduct even though it has moved in both directions through that tube. Moreover, when the inclosed egg is so small that it might lie either near the point or near the butt of the inclosing egg, it does as a matter of fact (Barnes, '63; Herrick, '99b, p. 410; Grivel, :02; and Kunstler et Brascassat, :02) always lie near the point, showing, since the egg moves through the oviduct point forward, that it is the second egg that incloses the first and not the reverse.

In one respect the egg obtained from Mr. Nielson is of special interest. As I have already stated, it contained no yolk in either the inclosed or the inclosing albumen, though traces of yolk substance occurred in the latter near the butt of the inclosed egg. This was due, I believe, to a ruptured and partially escaped yolk. If this explanation is true, is it possible that the yolkless condition of the inclosed egg is also due to the loss of its yolk? It seems well established, however, that albumen can be formed in the oviduct without the presence of a yolk. Possibly foreign bodies when introduced into the oviduct by accident may induce the production of this material. Von Nathusius ('95, p. 655) has pointed out that when a hen lays an egg, the distal portion of the oviduct is so far rolled out that foreign bodies may adhere to it and thus be carried well into its cavity. In this way chicken-yard refuse and feathers, such as have been noted by Landois ('82, p. 23) in eggs, have doubtless reached that position in the oviduct where incorporation in a forming egg was possible. These and like bodies may excite the oviduct to the production of albumen and thus give rise to a core around which a shell might be secreted.

Parasites are also known to make their way into the oviduct, and, though what have been supposed to be tapeworms in eggs

have in all cases thus far carefully inquired into proved to be mere clots of albumen or other like materials (Landois, '94), it is well known that distomes and threadworms do sometimes occur imbedded in egg shells (Landois, '78; Collin, '94). One of these might well form a nucleus around which albumen could be deposited.

But it must also be kept in mind that the ovary and the oviduct are not such independent organs as perhaps has been implied. Davaine ('61, p. 256) states that Claude Bernard opened a hen that had died after laying numerous small yolkless eggs and found the infundibulum closed and the body-cavity full of yolks. It is therefore probable that the simple activity of the ovary may in some indirect way excite the production of albumen in the oviduct, and it is my opinion that many yolkless eggs such as that described in this paper, are formed in this way rather than that they once possessed yolks and through some accident lost them. Such questions, however, are subjects for experimental investigation rather than for speculation.

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BIOLOGICAL RELATIONS OF CERTAIN CACTI¹

W. A. CANNON

ESPECIAL interest is attached to the study of the cactus family because it is peculiarly well adapted by habit and by structure to withstand the trying conditions of the desert. The greatest development of the group occurs in the arid portions of tropical and subtropical America, but the conception which this statement is likely to give that the cacti thrive best with a modicum of water and live in localities that are too severe for all other desert plants to endure, is erroneous. The cacti like other plants of the desert are most vigorous when the water supply is adequate, and it is by no means certain that such a form as the giant cactus (*Cereus giganteus*) or the barrel cactus (*Echinocactus wislizeni*), as well as the larger Opuntias, do not require a larger amount of water than many of the large plants of other families.

However, it is also likely that no desert plants can live and perpetuate their kind under more arid conditions than some of the cacti. The general means by which they accomplish this are too well known to require repetition here, but certain adaptations to desert conditions, not so well known perhaps, may be pointed out. The most important factor in the life relations of the desert plants is unquestionably the available water supply, and the most striking adaptations accordingly are associated with the absorption, the storage, or the conservation of water. This relation to the water supply, either in apparent independence of it or in intimate association with it, is met at each stage of development. For instance, the seeds of the giant cactus will germinate in summer while lying on the top of air-dry sand and without previous wetting. Seedlings of *Opuntia versicolor* are provided with water-storage organs (Fig. 1) although such are absent in the adult plants. The reaction of the mature plants to a variable water supply is also noteworthy. Specimens of

¹ Papers from the Desert Botanical Laboratory of the Carnegie Institution, No. 11.

Opuntia engelmanni, wrinkled from the loss of water during a long drought (Fig. 2), absorbed sufficient water within two days following a storm to make their joints plump and smooth (Fig. 3). The giant cactus is especially adapted by the peculiar for-

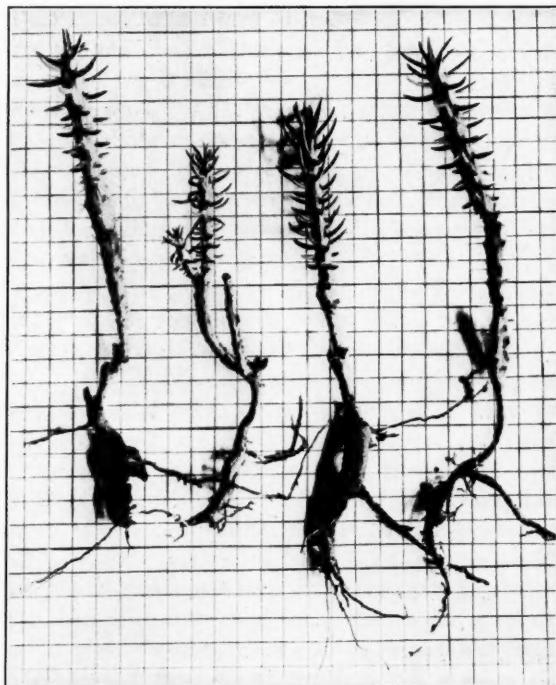


FIG. 1.—Young plants of *Opuntia versicolor* showing the water-storage organs — the swollen roots — which are not present as such in the mature form.

mation of the rind to undergo without injury considerable changes in volume which are induced by a variation in the water content (E. S. Spalding, :05). Other adaptations, such as certain characteristics of the root systems, certain peculiarities of structure and their relation to transpiration, and the transpiration under different conditions may be presented somewhat more fully.



FIG. 2.—*Opuntia engelmanni*, May 11. May 11 marked the close of a severe dry period and the wrinkled surface of the cactus shows that it was suffering from the drought.

THE ROOT SYSTEMS

A special study of the structure and extension of the root systems of desert plants cannot fail to be of great interest and importance in contributing to a right understanding of the biology of these plants. Many characteristics of the desert vegetation are without much question directly traceable to peculiarities of the various root systems. It has been observed (Coville, '93, p. 43) that the fairly equal spacing of desert shrubs is one of the characteristics of their distribution. The primary cause for this is presumably the struggle for water and their distribution is, therefore, an expression of the mutual relationship of the root systems. Again, frequently the form of the root is incompatible with certain habitats,—for example a subirrigated plant would find difficulty in growing where the subsoil is the rock-like *caliche*, — and plants with such deeply penetrating roots, for instance, are for this reason limited in their distribution. It is theoretically possible, and so far as I have observed actually true, that those plants that have a root system which is at once superficial and which penetrates the ground deeply, all other things being equal, may also have the widest choice of habitats. Certain it is that the creosote bush, for example, which has a root system of this character (V. M. Spalding, :04) is perhaps the most widely distributed of our desert shrubs. Although this view of the relation of the character of the root systems to the distribution of these plants is advanced tentatively only, the importance of it as a factor which must be taken into consideration in this connection and sometime carefully studied, is very apparent.

The root system of a specimen of *Echinocactus wislizeni* which was 60 cm. high and 35 cm. in diameter, growing about 75 meters north of the laboratory, was carefully exposed and the course of its roots mapped (Fig. 4). The roots, as the figure indicates, were branched very freely. There were three main roots which arose from the base of the plant not far from 10 cm. from the surface of the ground and which so directed their growth, and that of the branches, that the area compassed by them was about equally apportioned and well covered. As a rule the roots were

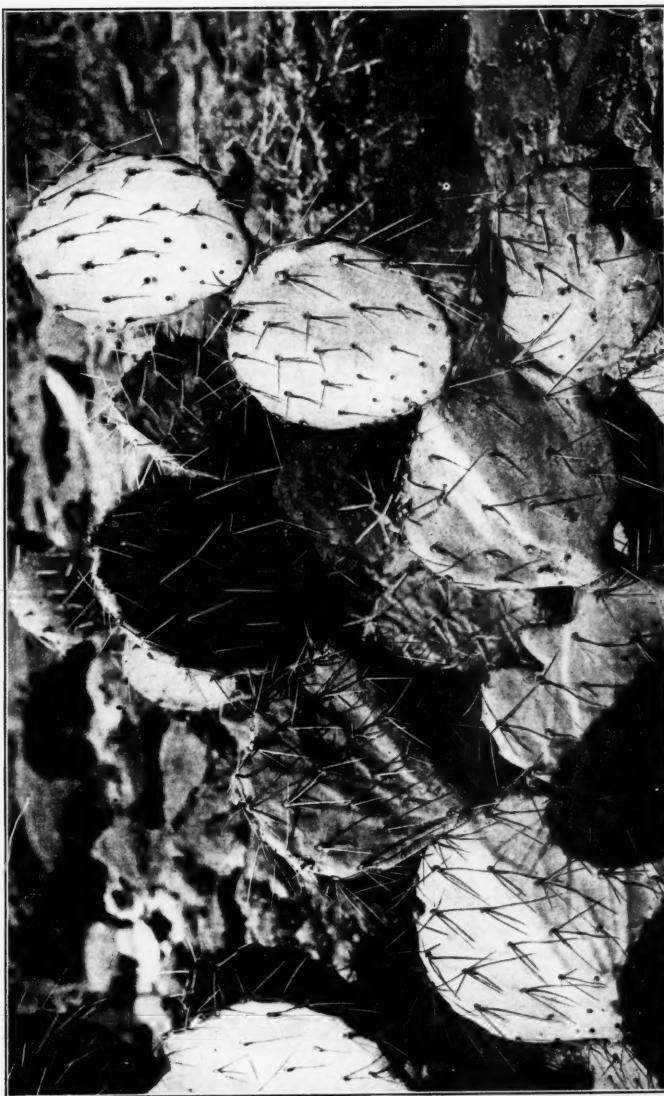


FIG. 3.—*Opuntia engelmanni*, May 14. This is the plant shown in Fig. 2. Rains came May 10-12; the plump condition of the joints of the plant on May 14 indicates that water was absorbed promptly after the rains, and in considerable quantity.

slender. At a distance of 15 cm. from the plant one of the largest of them was 7.6 mm. in diameter, and one meter from the plant it was 4.6 mm. in diameter. The roots ran about 6 cm. below the surface, in places which were free of stones, but when a stone was encountered the root dipped beneath it and availed itself of

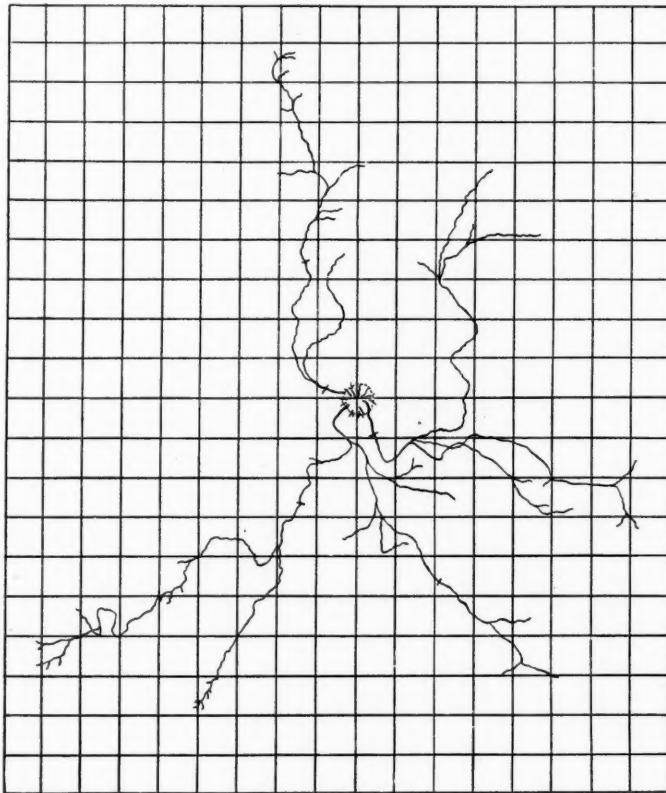


FIG. 4.—Root system of *Echinocactus wislizeni*. Scale: 1 unit = 30 cm.

the better water supply to be found there. The most deeply placed root, however, was not more than 10 cm. below the surface of the ground. There are therefore two noticeable characteristics of the root system of *Echinocactus wislizeni*, namely, the roots

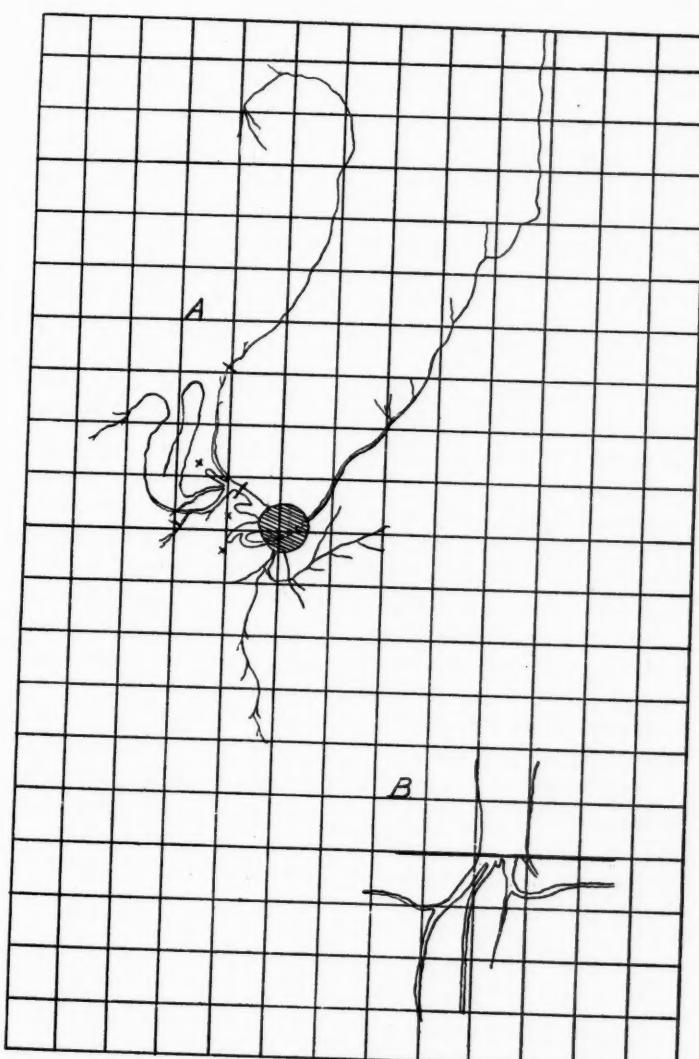


FIG. 5.—Root system of *Cereus giganteus*. Scale: 1 unit = 10 cm.

are slender throughout their entire course and they are superficially placed.

The roots of *Cereus giganteus*, on the other hand, in form and position, and perhaps in extent and branching also, are very different from those of *Echinocactus*. Fig. 5 represents the root system of a *Cereus giganteus*, about one meter high, which was growing 200 meters west of the *Echinocactus* just described. Four main roots were observed to arise from the base of the plant. At first they were relatively heavy, from 2 to 4 cm. in diameter at the proximal ends, but they became smaller very rapidly as the distance from the plant increased—in a manner much as is indicated by *A* of the figure. Very soon after leaving the plant the roots branched. One branch, whose later history could not be traced, struck directly downwards, and the other took a more or less horizontal course. The latter branched at intervals, although perhaps not so frequently as those of *Echinocactus*, and extended, in one instance at least, over one meter from the plant's base. How much farther the root reached could not be learned because of its fragility and the small size of the distal branches. The superficial portion of the root system of *Cereus giganteus* was more deeply placed than were the roots of *Echinocactus*, and owing to the fact that these parts were not so richly branched, the ground included by them was not so thoroughly covered. However, in one characteristic, which is of interest to note but whose significance I have not investigated, the superficial roots of the two forms are alike, namely, the longer roots and the greatest number of roots are situated on the uphill side of the respective plants. This peculiarity is shown in the two figures. In Fig. 4 the uphill side is to the right, and in Fig. 5 it is at the top of the sketch.

CERTAIN STRUCTURAL FEATURES

Perhaps the correlation of structure and function is nowhere more patent than in the peculiar transpiration-controlling and transpiration-promoting tissues, and the rate of transpiration in certain cacti. As is well known, the cacti are well adapted structurally, laying aside for the moment the matter of water storage,

to retain water for long periods. As an illustration of how long a specimen of *Cereus giganteus* must ordinarily retain water in this locality, the following may be suggested. Calculations based upon the known average water content of these cacti, on the estimated spread of the root system, and on the average rainfall at this place indicate that approximately two years' rainfall, assuming the rainfall to be normal each year, 11.74 in., are required to supply a cactus 15 feet high with an amount of water equal to what it usually contains. The necessity of husbanding the water so hard-gained is even more apparent when the relation of the normal rainfall to the usual evaporation is taken into account. As has been pointed out (Coville and MacDougal, '03, p. 27) the annual evaporation is 7.7 times the rainfall. This is the normal ratio, but when the precipitation is below average, as in 1904 (when it was 75 percent normal) the disparity is even greater (Cannon, '05b).

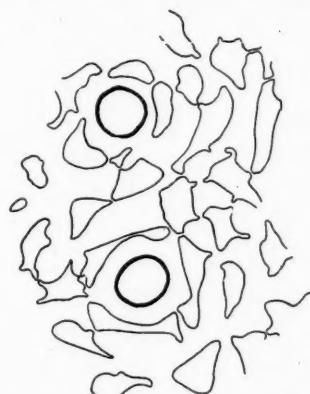


FIG. 7.—*Echinocactus wislizeni*. The heavy-walled supporting tissue lying immediately under the epidermis. This section was parallel to the surface.

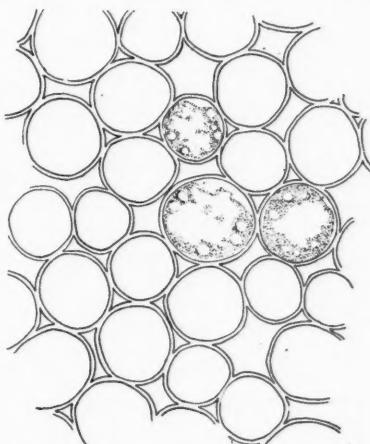


FIG. 6.—*Echinocactus wislizeni*. A portion of the cortex showing chlorenchyma with large intercellular spaces. The section was made parallel to the surface.

Although, as mentioned above, the structural adaptations for the retention of water are well known, reference should here be made

to the special adaptations found in *Echinocactus wislizeni*, par-

ticularly on account of the small rate of transpiration which was demonstrated in this plant. *Echinocactus* has a heavy outer membrane which is cuticularized (Figs. 9, 10). The stomata are peculiarly fitted to guard well a too rapid loss of water, and they have a somewhat complicated structure. The stoma as characteristic of many xerophytic plants, is sunken below the general surface of the stem, and from it in *Echinocactus* a tube, divisible into two portions of separate function, leads deeply into the outer portion of the cortex and becomes

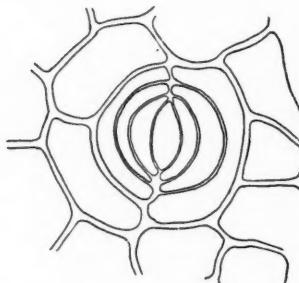


FIG. 8.—*Echinocactus wislizeni*. Surface view of stoma.

the peripheral portion of the extensive intercellular aërating system of the plant. The substomatal tube, really trachea, is shown schematically in Fig. 11. The outer portion, which is heavily shaded in the sketch, is cuticularized throughout its course in the sclerenchymatous tissue (Fig. 10) of the cortex, and in this part it acts merely as a tube for the conduction of gases. Beneath the supporting tissue the tube enters the chlorenchyma and its wall is no longer cuticularized; it here functions as the substomatal chamber proper.

It is of interest to contrast with this permanent structure a form of stoma and sort of adjoining tissue which are a part of evanescent organs, and which also appear to have somewhat different functions. I have reference to the stomata of the leaves of *Opuntia versicolor*. The permanent stomata of this *Opuntia* closely resemble those of *Echinocactus* but the temporary stomata, those of the leaves, are very

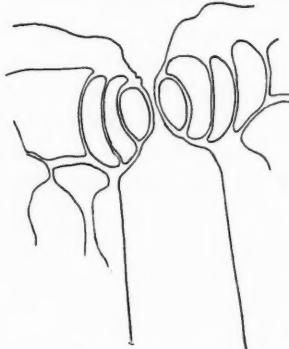


FIG. 9.—*Echinocactus wislizeni*. Stomata and substomatal canal which passes through the supporting tissue (see Fig. 7) to the more deeply placed chlorenchyma (Fig. 6).

different (Figs. 12-14). These stomata are superficially placed

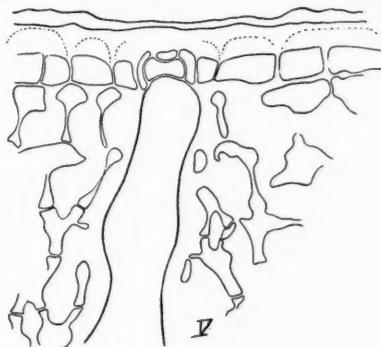


FIG. 10.—*Echinocactus wislizeni*. Same as Fig. 9, to show character of supporting tissue.

and they open directly into the substomatal chamber (Fig. 12). The substomatal canal of the permanent organ, therefore is lacking. Associated with this form of stoma is the absence of a sclerenchymatous supporting tissue, and, consequently, the extension of the chlorenchyma to the epidermis. The heavy outer epidermal wall of the older portions of *Echinocactus* is here replaced by a delicate one. In connection with this structure of the leaves of *Opuntia* appears their function of promoting transpiration, and presumably the respiratory activities as well, and in this they render it an important service, as will be apparent from the results of the transpiration studies.

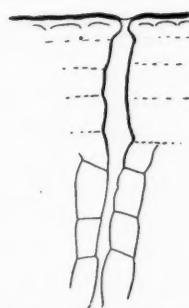


FIG. 11.—*Echinocactus wislizeni*. Semidiagrammatic sketch showing the relations of the substomatal canal. That portion of it which is in the supporting tissue is heavily shaded; that portion in the chlorenchyma is represented by a lighter wall.

TRANSPIRATION OF CEREUS AND ECHINOCACTUS

In all of the studies on the transpiration of cacti which were conducted outdoors the polymer method (Cannon :05a), described elsewhere, was employed. A few, however, which will be pointed

out later, were done in the laboratory by weighing in a manner

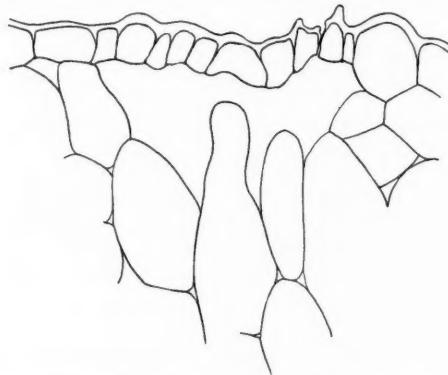


FIG. 12.—*Opuntia versicolor*. Cross section of a leaf showing two stomata, one of which was cut in two at right angles to the guard cells and the other parallel to and to one side of them. The confluent substomatal chamber is shown. This section is to be contrasted with Fig. 13.

to be described. This change in method was made necessary on account of the high relative humidity prevailing at the time.

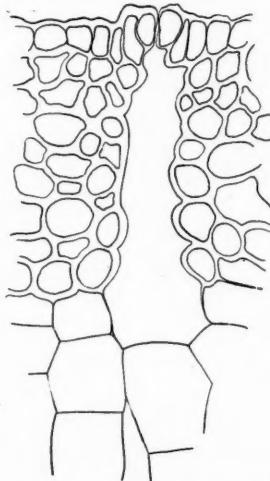


FIG. 13.—*Opuntia versicolor*. Substomatal canal and partly developed supporting tissue of a young stem.

Experiment No. 1. *Cereus giganteus*

Time	Percent of Saturation	Temperature	Amount in Milligrams
9:27 A. M.	42.0	94° F.	67.5 ¹
10:37 A. M.	48.5	105° F.	106.0

The cactus was located in the shade of a *palo verde* (*Parkinsonia microphylla*) on a dry mountain-side not far above the bed of Salvino Canyon, 18 miles east of the Laboratory. Higher

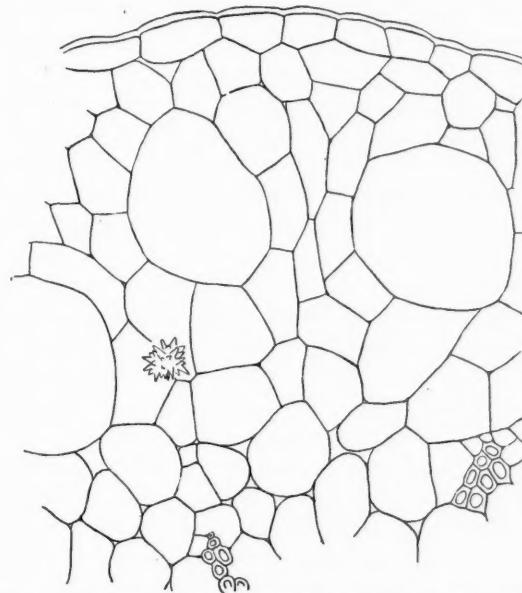


FIG. 14.—*Opuntia versicolor*. Cross section of a leaf to illustrate its delicate structure.

on the same slope were many other giant cacti of large size, and other typical desert plants such as *Encelia farinosa*, *Fouquieria splendens*, as well as other species of cacti.

¹ The first amount in each case is the absolute humidity of the atmosphere of the bell glass when the experiment begins. The second amount is the absolute humidity at the close of the experiment. The difference between the two is the amount transpired.

The transpiration of the cactus was taken September 9, and as the high relative humidity at the beginning of the experiment shows, the effects of the rains of August were still manifest. The rate which is 0.2 milligrams per minute for 100 sq. cm. of transpiring surface, may be considered a high one, since without exception the greatest rate of transpiration of all the plants, whose seasonal variation in rate has been observed, has been after or at the time of the summer rains.

Experiment 2. Echinocactus wislizeni

Time	Percent of Saturation	Temperature	Amount in Milligrams
10: 28 A. M.	32.5	82° F.	36.0
2: 05 P. M.	35.5	91° F.	51.0

This experiment took place on March 19, at the Desert Botanical Laboratory.

The rate of transpiration of the entire plant per hour is 3.4 milligrams.

On September 3 the experiment was repeated when the following data were derived:—

Experiment 3. Echinocactus wislizeni

Time	Percent of Saturation	Temperature	Amount in Milligrams
12: 24 P. M.	35	104° F.	60
3: 14 P. M.	45	109° F.	89

The rate for the entire plant is 9.6 milligrams per hour.

This specimen of *Echinocactus* is growing on a westerly slope on the Laboratory Mountain. In its vicinity are found a few giant cacti and *Encelia farinosa*, *Lycium* sp., and *Parkinsonia microphylla*. Because of the desirability of preserving this specimen the surface was not computed and therefore the rate cannot be compared directly with that of *Cereus giganteus* as given in the preceding experiment. However, it happened that the two cacti were of nearly the same size,—10 cm. in height,—and a general comparison between the two can be made. The rate of

Cereus per hour was approximately 33 milligrams while that of *Echinocactus* on September 3 was 9.6 milligrams. Whether this difference in the rate is constant for the two genera, or is attributable to other and unknown causes aside from the relatively slight difference in surface, is not known.

TRANSPERSION OF *Opuntia versicolor*

A specimen of *Opuntia versicolor* about 20 cm. high, growing near the laboratory building, was studied at various times during the dry portions of the year (1904), namely, in March, April, June, and July. The observations indicate that the reaction of *Opuntia* to water is very different from that of other desert plants, such for instance as *Covillea tridentata*, *Encelia farinosa*, or *Fouquieria splendens* (Cannon, :05a, p. 404), suggesting a unique position among its associates and an important factor among the varied ones that brought about the present distribution of the group to which it belongs.

As has been shown in another place, the transpiration of *Fouquieria splendens*, as well as that of other desert plants and plants of more humid regions (Burgerstein, :04), under certain conditions increases with an addition to the available water supply (see Cannon, :05b; V. M. Spalding, :04, :05). Thus after rains, but before leaves appeared, the rate of transpiration of *Fouquieria splendens* increased about three fold; after leaves had been formed and while they were developing the rate was relatively very great. A similar condition was likewise observed in *Encelia farinosa*, *Covillea tridentata*, and in other plants. In *Opuntia versicolor*, however (see Figs. 2, 3), and probably in other *Opuntias* the response to the rains is indeed also positive but in a very different way. The cactus absorbs water greedily, and as a consequence it at once increases in size, and its tissues become turgid. But, so far as I observed, the rate of transpiration did not increase proportionally. Indeed, laboratory experiments, in which a small specimen of *O. versicolor* was attached to a potometer by a long delicate tube so that the cactus could be weighed at intervals at the same time that its rate of absorption was being recorded, showed very clearly that under such

conditions the cactus may absorb water much faster than it gives it up by transpiration.

It should be noted that the specimens of cactus which were experimented upon both in the field and in the laboratory did not have an adequate water supply at their disposal previous to the times of the experiments. As a general thing not until some time has passed after the water has been absorbed, does new growth appear with its embryonic structure and its evanescent leaves and then only does the rate of transpiration become greatly increased. During the periods of drought the plants make but little new tissue. These peculiarities of *Opuntia versicolor* were observed repeatedly and will be presented in the succeeding résumé of representative experiments.

The transpiration of *Opuntia versicolor*,—an entire plant,—was as follows:—

March 25	51.0	milligrams in one hour
March 26	63.0 ¹	" " " "
April 25	19.9	" " " "
June 30	27.5	" " " "
July 4	26.1	" " " "

During the period from March to July the rainfall was unusually small and the cactus had an insufficient supply of water. The rates of March, April, and June, therefore, represent the transpiratory activities of the plant in times of drought. At various times in the midst of the dry seasons *Fouquieria*, *Covillea*, and other plants had been irrigated and the effects on their transpiration were recorded (Cannon, :05b). To learn how an increase in the water supply of *Opuntia versicolor* would influence its rate, as well as to learn how the rate under such circumstances would compare with that during dry conditions, it, also, was irrigated. On June 27, which was a time of drought, nine gallons of water were poured slowly on the ground at the base of the cactus, but it did not show by an accelerated rate (see the rate of July 4, above) that it had absorbed any of the water. That it had really done so, however, was indicated by the fact that the plant had become rigid by the increased turgescence of its tissues.

¹ Unfortunately a small branch was broken from the plant after this experiment so that the winter and the summer rates are not comparable.

The transpiring surface of the *Opuntia* was not estimated, so that its rate cannot be compared directly with the rate of other cacti or plants of other families although this, perhaps, is of minor consequence. The important fact was established that the plant does transpire measurable amounts of water even in the driest times and that it absorbs water quite out of proportion to its rate of transpiration.

I wish now to call attention to a phase of the biology of *Opuntia versicolor* which is also of great importance in the economy of the plant but which has hitherto received little emphasis, namely, to the *rôle* which the leaves play in transpiration.

On August 18 the polymeter apparatus (Cannon, :05a, Fig. 4) was adjusted to take the transpiration of a branch of the cactus which bore leaves and which was situated a few meters north of the laboratory building. The data derived from this experiment are as follows:—

Experiment 4. Transpiration of Leaves of Opuntia versicolor

Time	Percent of Saturation	Temperature	Amount in Milligrams
2: 20 P. M.	40	95° F.	62.
2: 30 P. M.	59	98° F.	101.

The branch transpired at the rate of 234 milligrams in one hour, or 0.91 milligrams per minute for 100 sq. cm. of transpiring surface.

As soon as the experiment was finished the surface to the stem was coated with vaseline and the experiment was repeated. The following, therefore, is the transpiration of the leaves only.

Time	Percent of Saturation	Temperature	Amount in Milligrams
2: 42 P. M.	41.5	97° F.	69.
2: 52 P. M.	52.0	100° F.	87.

The rate per hour for the leaves of the branch is 108 milligrams, or 0.42 milligrams a minute for 100 sq. cm. of surface.

The surface of the stem alone was estimated at 331 sq. cm.; that of the leaves at 97 sq. cm. Therefore with somewhat less

than one fourth the entire transpiring surface, the leaves alone transpired nearly one half the whole amount.

The high humidity at this time was unfavorable to the further use of the polymeter method so that the experiments upon the transpiration of the leaves of the cactus were continued with a special weighing apparatus in its stead. Since the results of all of these experiments were essentially alike, I shall refer to one of them only.

A branch of *Opuntia versicolor* with leaves was placed in a bottle containing water which was so arranged, with a capillary tube as well as the branch fastened in the stopper, that the air could enter and maintain a pressure within uniform with that of the room, while only an inappreciable quantity of vapor escaped. In one hour, 2 : 15 to 3 : 15 P. M., the branch lost 180 milligrams in weight. The stem was then coated with vaseline and in one hour, 3 : 30 to 4 : 30 P. M., the loss of weight was 100 milligrams, which was, of course, the transpiration of the leaves only.

There were 69 leaves on the branch whose entire surface was estimated at 55 sq. cm. The surface of the stem alone was 65 sq. cm. Therefore the leaves had about 45 percent of the entire transpiring surface and they gave off about 55 percent of the entire amount transpired.

SUMMARY AND CONCLUSIONS

The leading points in this paper and the conclusions may be briefly stated in the following summary.

1. The root systems of *Cereus giganteus* and of *Echinocactus wislizeni* which were studied and mapped, present characteristic differences. The root system of *Cereus* is in part superficial and in part deeply placed. The root system of *Echinocactus* is superficial only. There appears to be a relation between the character of the root systems of these plants and that of the habitats in which they naturally occur. For example, the form and the extension of the roots of *Cereus* inhibit its occurrence in localities where the underlying formation is of such nature that they cannot reach the usual or needful depth. We accordingly find the plant on rocky mountains, or where the soil is deep, but in this locality

it does not grow at all, or rarely, on the *mesa* where the rock-like *caliche* forms a thick and nearly impenetrable stratum which reaches almost to the surface. However, it may not be wholly a problem of anchorage, since the morphological condition may be associated with a physiological one, as for instance, subirrigation or proper drainage which may be indispensable factors in its water relations. Although the character of the root system may thus be closely connected with the character of the habitat, certain features in the local distribution indicate that it cannot be too narrowly insisted upon. For example, *Cereus giganteus* avoids northern slopes, although to all outward appearances the structure and the water supply may be quite the same as on the other sides.

Echinocactus presents quite a different condition of affairs. The plant does not require unusual protection against lateral stresses. It grows most abundantly in this locality on the *mesa* where the soil is shallow. The roots are so placed that they can neither afford safe anchorage for a tall plant, nor absorb water at the water level. There is therefore a direct relation between the character of the plant and that of the root system, on the one hand, and the character of the root system and that of the habitat, on the other. It should also be noted that the roots of Echinocactus, which are very shallowly placed, permit the plant to derive benefit from relatively small rains, but, by the same token, that they prevent it from getting water other than what falls on the area included by them.

2. The striking disproportion between absorption and transpiration, which was observed in *Opuntia versicolor*, is thought to be of great importance in accounting for the distribution of the plant (and perhaps of the family) in those parts where evaporation greatly exceeds precipitation.

3. A low rate of transpiration was demonstrated in *Opuntia versicolor* and *Echinocactus wislizeni* during periods of prolonged drought. At the time of the summer rains the rate was greatly increased and in all instances the increase was associated with the renewal of growth.

4. A direct relation was observed between structure and transpiration. The mature portions of Echinocactus and of *Opuntia*

versicolor are suited by the heavy outer epidermal wall, which is cuticularized, as well as by the stomata of peculiar structure, to resist rapid loss of water. This is the type of structure that is to be found during the periods of drought. The embryonic portions of these cacti, and the evanescent organs, in which are included the leaves of *Opuntia*, are well adapted to promote transpiration. This is accomplished in the embryonic tissues by a thin epidermal wall and by the undifferentiated portions of the outer part of the cortex by which a rapid transfer of water is possible. The substomatal tube functions also throughout its entire length as the substomatal chamber. In the leaves of *Opuntia* not only is the epidermal wall delicate, but the outer cortex is never differentiated into sclerenchyma and chlorenchyma and there is no substomatal canal. Such is the structure of the tissues at the times when the rate of transpiration is most active.

5. The leaves of *Opuntia versicolor* play an important rôle in transpiration. In one instance with somewhat less than one fourth the entire transpiring surface the leaves transpired nearly one half the whole amount. In another instance about 45 percent of the entire transpiring surface was foliar and the leaves transpired about 55 percent of the total amount.

DESERT BOTANICAL LABORATORY
OF THE CARNEGIE INSTITUTION

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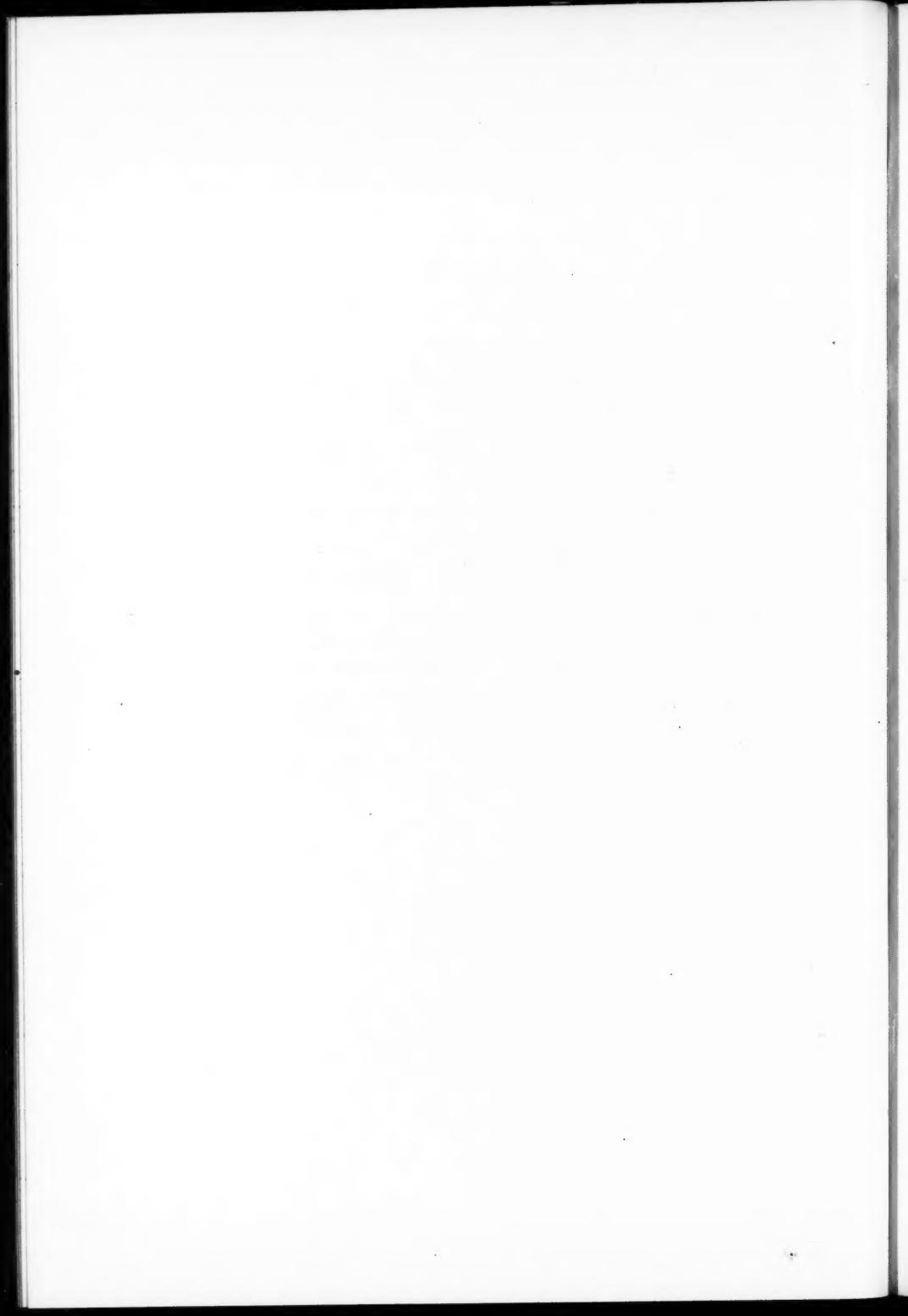
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CONTRIBUTIONS TO THE PHYSIOLOGY AND BIOLOGY OF THE DUGONG

H. DEXLER AND L. FREUND

THE accounts of the habits of the dugong that have hitherto been brought to the notice of naturalists are really limited to a detailed description by Klunzinger, who as surgeon on the "Koseir" in the sixties, collected these accounts of the dugong of the Red Sea from the Bedouins. The account in Brehm's *Tierleben* is taken from him. The few who before or after him have had to do with the subject, have added but little that was new. For this reason we are warranted in publishing some very recent observations based on the field notes of one of us (Dexler). In 1901, through the generous support of the Gesellschaft zur Förderung deutscher Wissenschaft, Kunst, und Literatur in Böhmen, Dexler was enabled to make a visit of several months to the Coral Sea, and there to attend personally to the capture of the dugong. Incidentally, also, the rare opportunity was afforded of observing minutely for forty-eight hours the habits of a captive dugong, and to investigate in life what has hitherto been impossible for us to find out in the case of this animal.

The low flat coast of East Australia is a favorite haunt of the dugong. Here are broad, shallow bays choked with sand and covered with water at low tide, and connected with the outer ocean by numerous channels and passages. Here is the plant-bearing sea bottom on which occur the so called "dugong grasses" that constitute the food of the dugong. These bottoms are the chosen pasture of the dugong and it is one of their permanent occupants, being directly dependent for sustenance upon places of this sort. Wherever there are these conditions, to which must be added sea water and a particular temperature, there the dugong will be found as all observers agree (Ruppel, Klunzinger, Finsch, Semon). Sea water is its native element and it is questionable if it occurs at all in brackish water at the mouths of rivers, as is

maintained by many, *e. g.*, Brown. But as Finsch has stated, observations on this point are lacking, and we ourselves observed nothing of the sort. It is not known by the old Queensland dugong fishers to enter brackish water, and that it seeks the fresh water of the river itself is out of the question.

During the day the dugong remains in the deeper waters of the outer ocean and only at night comes in through the channels previously mentioned to feed in the bay. This observation is corroborated by those of Klunzinger in the Red Sea, and by the statements of Semon and Finsch. With regard to its appearance at night, Klunzinger makes a noteworthy statement based on the accounts of the Bedouins. According to them the dugong is recognized at night not only by its "blow" and by the phosphorescence of the disturbed water, but also, they asserted, by three shining spots on the back, a fact of which he was repeatedly assured but which he found it difficult to believe. Krauss attributed this to the luminosity of the sea, but by Brehm it is explained as due to the sparkling in three places of the water that is disturbed in swimming, and thus made luminous. These three places would correspond well to the rounded ripples that would be made by the head, the middle of the back, and the caudal fin. Langkavel also cites Klunzinger's statement. No such phenomenon was observed in the Australian waters, a fact that may have been in part due to the slight degree of phosphorescence of the surface water at that time of the year (the Australian winter). However, in spite of the bright light of the tropical sea, it is highly probable that this is a faulty observation, and untrue of the dugong. For as it swims, its head first appears above water to take breath, rarely the back follows, while the tail is never shown. These movements might very well produce a more or less clear flashing when the water is strongly phosphorescent, but it would seem quite as impossible to identify the animal by this as to tell it by the noise it makes in breathing. Dexler hunted the animals in waters inhabited by both dugongs and dolphins, and in the tropical nights made careful observations for many hours while lying in wait at the nets in their pursuit. At no time, however, was it possible to distinguish the two by their resounding "blow" though often they were extremely near at

hand. Moreover, the blackfish hunters were questioned in regard to this point, and although they are extraordinarily expert in whatever has to do with the pursuit of their quarry, they were unable to distinguish between the sounds produced by the breath of these animals.

It is well known that the movements of the dugong in swimming are slow and clumsy (Klunzinger and Finsch). Thus late one evening Dexler observed from his boat, six dugongs that lazily broke water scarcely ten meters away, and with a forward rolling movement disappeared again. As a rule, only the nasal portion of the head came above water in taking breath as Ruppel also has observed ; but Ruppel and Semon likewise agree in stating that very rarely the anterior part of the body or the entire head may appear.

Before proceeding to describe certain peculiarities of biological interest, we may add the important observations made on a captive live dugong that have furnished the basis for the systematic treatment of the biological material to follow. More particular details as to capture will also be given shortly.

On one occasion when the nets were examined, there were found in them a large ray, an ocean-butterfly, and a dugong whose tail had become entangled in the net, though otherwise the animal was able to move freely. This might be called very unusual, for the animals generally strike the net headfirst, or with one of the flippers, getting entangled, and while hanging in the net they thrash and roll about until they are completely entangled by the cords. The heavy net is thus drawn together into a solid mass and the dugong, no longer able to raise it in order to come to the surface for breath, is drowned. In the present case, as the net was tightly drawn together, it could not be pulled up on the spot, and it was decided to drag the whole thing out onto the shore, with the captive in tow. The fast cutter had some difficulty in pulling the animal, but despite its tremendous struggles it was towed along after the boat, tail first in the gurgling wake. Twice the boat was stopped in order to allow the exhausted bull to obtain air, and on these occasions it became very excited and unmanageable. It kept constantly trying to dive, jerking its arched body into the deep, and when it

felt the pull on its tail it rolled over and over on its long axis two or three times. Repeatedly during these movements, it voided gaseous and solid excrement which diffused the characteristic odor of the dugong. The captive came up to breathe at intervals of from 14 to 42 seconds, exhaled with a long and forcible "blow" and inhaled with less noise and more quickly. Then the diving and rolling were repeated.

With the flood tide the boat was brought over the bar to the mouth of the Wallumkreek and there anchored. Here in the shallow water the animal became for the first time somewhat quiet after the people had withdrawn, but when the dingies put off from land again and approached it, straightway the diving and struggling began anew. From this it seemed that it was not so much the sight of the approaching boats as it was the sound that acted upon the animal's consciousness. If a black cloth or a piece of white canvas were waved above its head, the animal was not visibly disturbed or only rarely. But if one stamped on the deck then immediately it thrust its nose into the sand in a reflex diving or escaping movement.

It was a simple matter to secure the dugong, for the helpless animal had hardly more than its great weight with which to oppose its capture. A strong line was soon tied about its tail and made fast to the shore, and the dugong was then rolled out of the net into the water. There it appeared to feel better and began at once to thrash clumsily about, and tried to hide underneath the cutter. However, the strain on the line soon relaxed and with but slight exertion the dugong could be dragged into shallow water. By this time it put its snout above water for breath at intervals of from 17 to 65 seconds and breathed in the manner above described. If anyone spoke, or struck the tiller, or rattled the anchor chain, or made any other noise, the dugong started off violently and tried to dive deeper with a movement like that of the dolphins that roll head foremost over the surface of the water. In these attempts the animal repeatedly struck the sand bottom with considerable force. As soon as the line drew taut and the dugong felt the pull, it at once executed those remarkable rapid revolutions on the long axis of its body so that the line began to crack. When the noise ceased, the dugong soon became quiet and

lay motionless at the bottom, resigned to its fate. The intervals between breaths also became longer; at first they were from 43 to 60 seconds, then later, from 100 to 120 seconds apart. The following intervals were particularly noted: 104, 43, 60, 58, 95, 45, 105, 145, 85, 52, 50, 56, 120, and 85 seconds. The longest interval was 145 seconds though of course it must be kept in mind that in this case the animal was living under abnormal conditions, of which it must have been sensible, despite its partial freedom. The exhaled air had the same aromatic odor that is peculiar to the flesh, the excreta, the fat, and the steam from the boiling meat.

Thus the captive was studied for a number of hours, though but little else was brought out, for the radius of movement of which it availed itself was very small. Such observations as were made, however, were concerned almost entirely with the breathing. If one struck the animal with the tiller, it took each stroke with a slight shudder of the entire body. A more delicate sensitiveness of the skin could be made out at the corners of the mouth only. If one touched this region with the thumb, the dugong suddenly raised its head a hand's breadth from the bottom.

At evening the animal was rolled up onto the shore. During this process it thrashed about but little and allowed itself to be borne to land as quietly as a barrel. Not until it was choked did it repeat the tremendous strokes with the tail; at other times it moved neither the hinder part of the body nor the flippers. The latter were held against the breast, but if they were pulled away from the body, they remained in that position. For forty-eight hours it lay motionless. Its death was easy, for it was asphyxiated by quickly thrusting two gun wads into the nostrils during an inhalation. It raised its head, gave three mighty strokes with its tail and expired.

After this description, necessitated by the nature of the case, the biological details will again be systematically discussed. A little has already been said in regard to the manner of inspiration and expiration. Both processes take place out of water and exclusively through the nose. When the captured dugong was asphyxiated by closing its nostrils, it made no attempt to open and breathe through its mouth. These conditions in the dugong, it is interesting to observe, correspond exactly to those found

elsewhere in which the epiglottis is behind the velum or in front of it, and thus does not permit of a supplementary breathing through the mouth also (see Boenninghaus, '03, p. 84). Still, a similar topographical relation of the parts in question does not correspond to similar physiological phenomena, since in the Sirenia the velum palatinum and the epiglottis are very short (dugong, Owen, '38, p. 36; manatee, Waldeyer, '86, p. 245; Murie, '70, p. 178).

The dugong takes breath quickly, closes its nostrils, and sinks into the water. The closing results from the fact that the base of the nostril is raised as a slight eminence and pressed tightly against the top. It is interesting to observe that this procedure was also kept up while the animal was lying on the shore, and after each inspiration the nostrils were fast closed until the next breath was taken. According to Finsch, the closing of the nostril takes place through the action of a muscle which he does not indicate further. Ruppel ('34, p. 101) makes the incorrect statement that the nostrils can be hermetically closed by valves opening inwards and this fallacy is repeated by Brandt ('46-'69, p. 272). It is worthy of note that Turner ('94, pp. 319, 322, 326) found in the embryo and in the head of an adult dugong, valves ("valve-like flap," "plug-like valve"). No trace of such a valve can be demonstrated. The same structure, it has been claimed, has been found in the manatee. Brandt has previously asserted that in the Sirenia "aperturae nasales valvulis claudendae esse." Garrod ('77, p. 139) plainly speaks of a "flap valve" which forms the base of the nasal passage during the act of breathing but rises and completely closes the nostrils when it is shut. Likewise Brown ('78, p. 292), Chapman ('75, p. 461), Crane ('81, p. 457), and Noack ('87, p. 297) speak of a valve. Murie ('80, p. 32) had already rejected Garrod's statement and described the arrangement in the manatee as similar to that in the dugong, that is, that the base of the nostril is raised by contraction of a circular muscle and the opening is thus closed and that a free valve is out of the question.

In expiration there is a perceptible sound, that has been described above as a loud, long "blow." Klunzinger calls it a puff, Finsch a breath and a puff. Semon speaks of it as a singular hollow puff. Moreover, it is not accompanied by a discharge of vapor.

The increase in the length of the intervals between breaths, given above, is of interest. It is of course quite possible that the conditions of living and of breathing incident to the animal's long period of captivity were extremely abnormal, so that it is difficult to learn from them precisely the normal conditions. Nevertheless, we cannot be far wrong if we take as the average time between breaths a minute or a trifle less. The accounts of writers show great disparity on this point. Ruppel states that the dugongs come to the surface about once a minute. According to Klunzinger they do so every ten minutes and always about four times. Semon observed a large male that came up at intervals of from three to five minutes, and Finsch agrees as to these longer intervals. However that may be, it should be observed that the longest interval between breaths noted by us did not exceed two and a half minutes.

The dugongs, as above noted, feed chiefly by night and the same is also true of the manatee. To be sure, Noack ('87, p. 300) writes that they eat all day long without interruption, though Brown ('78, p. 295) had previously shown that as with nocturnal animals in general, they appear to feed at night only (see also Murie, '80, p. 24). The food and the feeding habits of the dugong have hitherto received very meager treatment. The animals lie, it appears, directly on the sea bottom, and with their thick lips graze leisurely upon the seaweeds (sea algae, according to Ruppel) that grow on the rocks or the sea floor, or they tear them from the bottom. Klunzinger speaks of sea plants, phanerogams (Niades). According to Finsch and Semon it is chiefly sea grasses and species of *Fucus*, according to Fairholme, "grass-like seaweeds," that constitute the food supply. But it may be stated that the food of the dugong does not consist of the thick, dark brown seaweeds floating up free from the bottom, but rather of the two green phanerogamous plants which were present in the stomachs of all the dugongs killed, unmixed with any other vegetable remains. The dugong fishermen employed by Dexler paid no attention to the beds of thick, dark brown seaweeds that could be seen growing up from the depths, but kept a sharp and constant watch of the clear sand that was almost without vegetation. There only were traces of the dugongs to be found, and never in the rank forest of seaweeds.

At our request Professor Aschersohn of Berlin, had the kindness to examine the species of plants found in the stomachs of the dugongs, and determined the one to be *Halophila ovalis* (of the Hydrocharidaceæ) and the other a species of *Zostera* (*Zostera capricorni* Aschersohn). The latter grows as a dense or scattered low bed, while in *Halophila* the root stalk and petiole are hidden in the sand and only the small leaflets project slightly. These two plants do not occur on rocky bottoms. The *Halophila* growth is limited to a depth of about six meters below the low tide mark; higher up, the growth becomes sparser until it entirely disappears. The white sand, peculiarly flecked and spotted by the *Halophila* leaves, is the especial feeding place of the dugong. The slight depths at which the dugong's food plants can grow also explain why the animal spends part of its life in the shallow seas and part in the off-shore waters.

Among the beds of *Halophila* are the so called dugong's tracks which at once show us how the animal takes its food. The tracks are long, curved, or wavy furrows in the white sand, with sides parallel, about four fingers broad and from four to six cm. deep. They are completely denuded of the *Halophila*. Their age is determined by the condition of their edges; fresh tracks have raised, sharply defined edges rising from one to two cm. above the sea bottom. If the strong flood tide has swept over them once, the sand becomes washed away, the edges obliterated, and the depth less, until finally they are entirely washed away. These furrows, showing white through the dark water, indicate the presence of the dugong. They are produced by the dugong passing over the *Halophila* beds as it feeds, pulling up the plants with its palatal processes, seizing them and perhaps washing them free of sand and other extraneous matter after the manner of certain waterfowl (ducks), and then chewing them up with its molar teeth. The dugong trails are not made up of separate marks which would indicate that the food is not plucked or bitten off in tufts. Such plainly continuous trails could not be made by the animal's lying sluggishly at the sea bottom. Also the peculiar lateral mark made by the tusks¹ in case of the male is

¹ Finsch believes that the tusks serve for uprooting the sea grasses rather than as weapons, but the fact that they are lacking in the females, argues against this view.

explained by the method of feeding just described which necessitates their active share in this process. Finsch, however, speaks of dugong tracks that were found on bars left more or less dry at low tide and could be readily recognized by the cropped sea grass, the disturbed bottom, and the imprints that were left by the animals' bodies. The first of these marks is established by the description preceding, but we may very well doubt whether the last is possible. Nothing of the sort was certainly observed in Moreton Bay, Sandy Straits, Wide Bay, and the northern Coral Sea.

There can be no doubt that the sensitive upper lip plays an important part in the taking of food. We know from the numerous careful observations made on living manatees in aquaria (we may mention only Brown, Murie, and Noack) how extensively these animals use the upper lip in feeding. It is unnecessary to lay particular stress on the fact that the food plants of the manatee are considerably different from those of the dugong because of their different habitats. The feeding habits of the dugong, as described, likewise differ in a general way from those of the manatee.

The washing of the food plants in the mouth must be very thoroughly done. Moreover, the dugong in its progress stirs up a quantity of sand and other inorganic particles but for all that, matter of this sort is seldom found in its stomach contents.

It has been previously stated that the captured dugong while being pulled ashore, voided excreta and intestinal gases with the characteristic dugong odor. It likewise did the same while lying hauled out on the land. The faeces were rather solid, cylindrical, and greenish yellow to greenish black, aromatic but not foetid; in the water they sank at once. Brandt ('69, p. 235) on the other hand, states that the intestinal contents of the Sirenia are strongly foetid, that the excreta float on the water, and are similar in form to those of the cow or the horse. Chapman ('75, p. 460) records that the faeces of a captured manatee that appeared to be suffering from constipation, were very hard, and that a constant stream of gaseous bubbles was given out from the anus. Murie ('80, p. 22) in case of his manatee merely mentions the droppings. Noack ('87, p. 300) on the other hand, states positively that the

excreta of *Trichechus senegalensis* appear greenish brown and rather formless, and that they are usually seen in the water only. Brandt's description agrees with neither the dugong nor the manatee. The urine of the dugong is clear as water.

Unfortunately no exact observations could be gathered as to the temperature of the dugong. The captured specimen, when brought out on land grew remarkably cool. The rectum showed a temperature of 19° C. Immediately after its death the thermometer was thrust into a cut made towards the base of the heart. The dark red blood, laden with carbon dioxide, that gushed forth had a temperature of 17° C. One can therefore hardly express an opinion as to the normal temperature of the dugong, especially as the temperature of the air (taken by whirling the thermometer under a clouded sky) was from 12° to 18° C., and the animal had lain motionless for 48 hours. In addition the heavy body, weighing some 192 kilograms, pressed the easily fixed sternum hard against the heart, whereby the activity of the latter, the blood circulation, and consequently the body temperature must have been very much disturbed. The marked increase in the interval between breathings is also evidence for this. The beating of the heart could not be detected either by its palpitation or by auscultation. This dugong impressed one as an animal that had become cooled off, and whose temperature conditions (irrespective of its abnormal position on the shore, and the unusual pressure) were so sensibly disturbed that it had lived only a short time after being taken from the water. Moreover, observations on manatees taken and kept in captivity, show that almost always changes in temperature are the cause of the sickness and death of the animals (Chapman, '75, p. 461; Murie, '80, p. 23; Crane, '81, p. 460).

No voice or production of a sound on the part of the dugong could be detected beyond the blowing already mentioned, that accompanies exhalation and inhalation and it is similar to that of the dolphins. Finsch leaves the question open as to whether or not there is a real voice. The earlier writers speak of a hollow moan or snort in the case of adult animals, and of a short, sharp cry frequently repeated on the part of the young (Brandt, '69, p. 235). However, Brandt thought all these sounds were pro-

duced by dying animals only, for Steller likewise stated that the uninjured rhytinas never uttered a sound, while the wounded ones gave a sort of hollow moan. Also in the case of the manatee no trace of a voice has been observed (Murie, '80, p. 22) though Murie believes that this may be different under other circumstances. Nevertheless it is to be remembered that the Sirenia have no vocal cords and that therefore sounds or noises may perhaps arise through vibrations of the laryngeal cartilages and should thus presumably take their course out through the nose.

As regards the activity of the sense organs the dugong as well as the manatee has reached a considerable degree of development. To many sense organs we are unable to assign a very definite function. This is true of the senses influenced by chemical stimulation, particularly the organ of taste, although as Gmelin ('92, p. 18) has shown, a taste organ is found in the manatee in the form of a so called papilla foliata. The care shown in the selection of food likewise points to a certain taste function. It is still more difficult to demonstrate the function of the organ of smell since the olfactory region is a nasal tract that is hermetically closed when under water. And yet Chapman claims that in conformity with the well developed bulbi olfactorii, the sense of smell must be very keen, for it was observed that his captive manatee seemed to become aware of food thrown in the water through its sense of smell rather than by any other sense. Brown ('78, p. 295) says the same, referring to Chapman.

Boenninghaus (:03, p. 91) has pointed out that mammals that seek their food in the water, can make no use of their sense of smell in obtaining it because the nose is tightly closed in diving. Even if this were not the case, he continues, mammals would still be unable to smell under water since the necessary stimulus for their olfactory nerves consists of scent particles which are held in suspension in the air and not, as with fishes, those that are suspended in the water. The disuse of these organs in the case of whales has led to their partial atrophy or total disappearance. Thus in the toothed whales the olfactory nerve is lacking, and correlated with this is the reduction of the ethmoid bones. In the Sirenia, however, in spite of a reduction in this bone, an olfactory nerve and bulbus are well developed, and this, too, not-

withstanding the objections just mentioned to the possibility of a smelling function. This possibility becomes a probability if not a reality by the observations on the manatee given above. From these observations it follows that in the Sirenia, at least, there is no connection between the reduction of the exethmoids and that of the olfactory nerve as there may be in case of the whales. It is also clear that the olfactory nerve as a functional structure must be capable of perceiving chemical stimuli that utilize water as the carrying medium instead of air as with land mammals. This assumption, of course, implies not only a partial functional adaptation of the olfactory nerve but it also limits such an adaptation to the herbivorous Sirenia alone. Finally we must also assume that the chemical stimulus, since it cannot reach the olfactory membrane in the usual way, must take another course, perhaps by way of the mouth, pharynx, and choanæ.

One more observation on the reflex irritability of the nasal membrane may be added here. Before the death of the captive dugong, Dexler endeavored to squirt water into the animal's nose as it drew in a breath. Instantly there came so violent an expulsion of the fluid — which had barely entered the nostril — that it was blown into a fine spray. The reflex was astonishingly violent and quick, and after it had taken place the nostril openings were fast closed as before. Thus the breathing movement that had been begun, was not completed. Surely so prompt a reaction is of the greatest importance for aquatic mammals. The action by which the nose is closed in case of the dugong is thus two-fold: voluntary and reflex. Water pressure as an aid is not necessary, or at least only auxiliary.

The dugong's sight must be characterized as rather poor. Otherwise it would be impossible to capture the animal with so roughly constructed a net, as is actually done. Nets from 80 to 150 meters long and from 5 to 6 meters deep are so disposed over the dugong's feeding grounds as to form a vertical wall. If a dugong comes in contact with the net, then it is usually all up with him; but a dolphin or a shark is never captured in so simple a manner. To be sure, the dugongs avoid the nets more easily on clear nights and for this reason the best time to take them is during the nights of the new moon. The cords of the

net are colored brown also. But if one is lucky he may capture the animals during the full moon as well, and even with new nets of shining whiteness. A further proof of the poorly developed vision is the fact that the captive dugong did not notice very much the waving of a white or a black flag and that not infrequently no notice was taken by the dugongs of the noiseless approach of the boat so that it was possible to observe these animals at very close quarters. Semon has remarked the same thing and the fishermen employed by Dexler asserted that they had repeatedly sailed right over feeding dugongs. This might be due, however, to the fact that it does not seem particularly shy (Semón) or that it is but little concerned for its own safety (Finsch). Feebleness of vision has likewise been recorded in case of the manatee (Chapman, '75, p. 454; Brown, '78, p. 295).

Investigation of the eye in case of our captive dugong was very difficult and therefore inadequate. For in order to do this one must lie directly beside the animal on the sand, and as may be readily seen, this is, to say the least, disconcerting when one does not know what movement the creature may make next. Moreover, the focal illumination was considerably hindered because the bulbi were deep set and constantly held pointing slantwise forward and downward, and the slit between the eye lids was narrow. Furthermore there flowed from the conjunctival sac such a quantity of tough, stringy secretion that the eye opening was constantly obstructed as with a thick plug.

This had first to be removed and the short intervals before its re-formation were utilized in a rapid study of the front portion of the eye. It was impracticable to expose the bulbus by opening the eye lids with one's fingers for these were closed tightly at the slightest touch. If they were forcibly pressed apart the nictitating membrane was then pushed up and the bulbus retracted in such a way that it disappeared under the membrane and the periorbital masses of fat that were forced forward. After five or ten minutes there appeared at the bottom of the fatty funnel thus formed a bit of the cornea like a small black spot which became no clearer in spite of long waiting. When the finger was removed the eye returned to its proper place, but for hours after it remained more deeply in the socket than it had

been before the investigation. This retraction movement took place independently of the position of the other bulbus.

By combining a number of partial observations, only the following particulars could be made out: a very strongly curved cornea, a considerable mobility of the blackish brown iris which reacted promptly to light, an indistinct radial striation, and a circular pupil.

The use of the ophthalmoscope seemed at first to be attended with no results, for nothing but a deep black was visible. Then at a chance motion of the dugong's eye the small, round rose-colored papilla or the disc of the optic nerve came in sight. Thereupon further exploration of the retina was made though without additional results. The entire area of all four quadrants consisted of a deep black continuous tapetum nigrum in which no details could be made out, as is likewise true to some extent in case of the ventral portion of the fundus of the horse. In order to perceive clearly the few extremely delicate capillaries that radiated to the tapetum from the upper rim of the disc required an increase of the observer's myopia from $2\frac{1}{2}$ to 5 dioptria from which may be deduced the degree of short-sightedness of the animal when out of water. The refraction of both eyes was the same.

It is noteworthy that the dugong also exhibited a certain dislike for bright light. At least one may so conclude from the eagerness with which it sought the shadow cast by the hull of the cutter.

In the manatee the pupil is round in life, but is transversely oval when the animal is dead (Murie, '80, p. 24). The dugong's pupil did not change shape after death. As in the manatee the nictitating membrane is well developed and freely movable. The eyelids are contractile and can be brought together until there is only a small slit, 12 mm. long, that remains not quite closed. The complete isolation of the bulbus from the exterior is effected by the nictitating membrane and the periorbital fat. The lids when closed are deeply wrinkled in a radial fashion, on account of the very strong orbicular contraction. This fact disposes of Pütter's (:03, p. 369) assumption based on the study of an embryo dugong whose eye slits were 5 mm. long and 3 mm. broad,

that the eye lids as in case of the whales cannot be opened nor approximated. The copious secretion of the conjunctival sac has already been mentioned. In our captive the string of slime was of the consistency of the white of an egg, was as thick as one's finger, and flowed over the entire eye and down to the ground. That this secretion might be due to the irritation caused by the atmospheric air or the unaccustomed light immediately suggested itself, but in all the animals taken the secretion was similar without any difference in regard to mass, thickness, consistency, nor transparency and thus possessed properties not present in a catarrhal flow. The corneal reflex was easy to demonstrate and the eye lids were always held open.

The eyes of the dugongs that were dead when taken, were always injured even if they had hung in the nets only six or eight hours. In such cases there were uneven scratches with finely serrated edges, spreading superficially or even onto the membra descemeti. They were always interior to the poles of the cornea, which lay free in death. The lens usually showed star-shaped to diffuse whitish cloudy spots that generally became more pronounced the longer the body was allowed to remain in the water. These spots were doubtless phenomena incident to death. The secretion of so great a quantity of slimy viscous matter from the conjunctiva is a protective adaptation to keep the surface of the eye from injuries from the sea water or its plankton fauna. If the slimy covering fails then the bulb of the eye is without protection. In this way are caused the scratches above mentioned, of whose traumatic nature there can be no doubt. In some specimens this even results in the eye lids becoming so abraded that they bleed freely.

The presence of a slimy protective coat for the eye has also been demonstrated for the whales, only in them it is not watery and mucous as in the dugong, but oily for otherwise it would be too easily washed away by the sea water (Pütter, :03, p. 370). Thus in the Cetacea a change of function has taken place in the glands of the eye as an adaptation to the conditions of aquatic life, and their products are likewise changed.

The copious secretion of mucus from the dugong's eye has long been known to many of the islanders of the Malay archi-

pelago. All sorts of fantastic tales are associated with the so called dugongs' tears, in regard to their use as a powerful charm. Brandt ('69, p. 274) speaks only of the young dugong's shedding tears and that these were carefully gathered because they aroused in the possessor an affection as strong as that of the female dugong for its young. This belief was taken advantage of by the German and French perfume makers in order to assure a better market in Java for their perfume imported under the name of "dugongs' tears," "ajer mata doejoeng" (Dexler, :05, p. 200).

In contrast to the senses hitherto mentioned, that of hearing appears to be very well developed in the dugong. Both in the water and on the land the animal seemed to be much more affected by sound impressions than by those of sight. The dugong lying on the beach winced considerably at a sharp squeaking sound made by drawing in air between the puckered lips, as do also the guinea pig and the mouse. Only after numerous and quickly repeated stimuli of this sort did it remain motionless. Whether its ear was acoustically over sensitive with the absence of water pressure could not be determined. Nevertheless some sort of a stimulating influence is within the range of possibility since the meatus acusticus externus de norma transmits the pressure of the water to the ear drum. Also in the great size of the ear bones is seen an adaptation for hearing under water and not in the air, just as in whales (Boenninghaus, :04, p. 282). Evidence for keenness of hearing in the dugong is also found in Finsch's statement to the effect that in setting out the hunting-scaffolds for capturing the dugong, one must work with great care since the slightest rattling of these is said to be enough to frighten the animals away. Also in harpooning them one must proceed with absolute silence (Fairholme). Dexler's fishermen gave the floats of their nets a wedge-shaped form in order, as they claimed, to obviate the slight noise produced by the waves in striking floats of any other shape, and so frightening the animals away. But as a matter of fact the dugongs were rather frequently captured when the floats splashed loudly.

Besides the sense of hearing, we must also accord a fairly good development to that of cutaneous sensibility. The large upper lip appears to be very richly provided with nerves of feeling and

as is evident from its anatomical construction, it is especially adapted for a manifold touch function, a fact that is of the greatest importance in the procuring of food. Apparently it serves in some degree to offset the seemingly poor eyesight, so that the animal in its wanderings over the beds of seaweed may be acquainted of the presence of blocks of coral, stones, and the like. The finely developed sense of touch in this organ is also shown by the fact that it was never found to be injured, in contrast to the skin of the rest of the body.

The presence of numerous scars and scratches on the skin of the back and sides of the body is characteristic of the dugong. As to their origin one can only guess. A very small portion of them may be set down as due to the wounds from the tusks of the males. Even in the oldest of 25 specimens examined, these tusks projected only 32 mm., and on their median sides they were almost entirely covered by the palatal processes. They are therefore very poorly adapted for making wounds. Possibly the animals wounded themselves in their endeavors to scrape off clinging parasites. The fishermen give a different explanation: they maintain that they have often seen the dugongs while at play, wound their backs on stones and corals as they rolled or swam about on the bottom. They thought the dugongs did this particularly over beds of *Pinna* shells, but it was impossible to determine this point.

There is little to be said in favor of the much discussed intelligence of the dugong. We have thus far seen no convincing evidence for according a particularly high degree of acuteness to a single one of the animal's senses. The senses of hearing and touch may be deemed the most highly developed; then, in a descending series, sight, taste, and smell. But even the keenness of hearing accorded the dugong cannot be compared with that of a really very keen-eared animal; otherwise the fact would be inexplicable that it is possible, though rarely to be sure, at times to sail up to feeding dugongs. When Dexler saw the herd of dugongs, previously mentioned at Moreton Bay, his men were rowing with splashing oars and without particular precaution. It must also be recalled that the often emphasized sound stimulus in water may frequently be communicated by the sensitive skin

and mucous membranes of the head as well. The other senses, as before mentioned, are much less well developed. Considering the manifestly slight capabilities of the peripheral sense organs, it can hardly be concluded that the central nervous system has reached a high state of development. In fact, the brain of the dugong is not only relatively small — $4\frac{1}{6}$ to $6\frac{1}{6}$ of the weight of the body — but as we shall show in a later paper, it is so lowly organized that only a comparatively slight degree of intelligence may be presumed in this animal. A further proof of the stupidity of the Sirenia is found particularly in their behavior towards enemies, in their defencelessness, and in the ease with which they may be killed and exterminated.

Another question which we will here treat at further length is that of the use of the flippers. While on the one hand it is assumed (Freund, :04, p. 346, and elsewhere) that the flippers of the Sirenia are used as paddles and balancing organs, on the other hand it is supposed (Abel, :04, p. 186, etc.) by way of explaining certain anatomical changes, that their function is that of supports. In the dugong, particularly in case of the specimen that lay on the shore, one could clearly see that movement from place to place in the water was accomplished solely by means of the caudal fin. The pectoral limbs were held motionless at the sides, and directed backwards. If one endeavored to roll the dugong over on its side, it began to strike out with its tail but allowed its pectoral limbs to remain where they were. Active movements of the flippers for the purpose of locomotion on dry land were not observed.

There has been very little recorded as to the movements of the dugong on dry land in general, where a use of the appendages in locomotion is to be presumed. Klunzinger makes only the indirect statement that in the northern part of the Red Sea they are frequent in winter especially in December and January, and that otherwise they seldom go to the land. Finsch (:01, p. 10) speaks of an account by Leguat who was led to Rodriquez, in 1691, mainly on account of a dugong that was seen lying on the shore. Still, in Finsch's opinion this was a stranded animal since the dugong does not voluntarily go onto the land, and moreover has nothing to seek there. If a dugong be captured and

thus forcibly brought to land, it may live 24 hours he was told by old Kabury on Palau. This observation is corroborated by that on our dugong which passed an even longer time — 48 hours — on land. It appears therefore that the dugong has not been actually observed to come out onto the land of its own accord, and in our opinion such a procedure is out of the question since the pectoral limbs are insufficient to enable the animal to do this. In no wise can it be established, as Brehm would have it, that "one can at least assume that those dugongs that have been seen lying on the shore were left by the ebb tide and were too sluggish to push their heavy bodies into the water again, preferring rather to wait quietly the next flood tide." They are simply incapable of using their anterior limbs, even as supports, on dry land. The possibility still remains that they support themselves on their flippers while gathering their food in the water. But against this supposition, in the first place, are their observed manner of feeding and the so called dugongs' tracks. In the second place, the difference in specific gravity between the animal's body and the supporting medium, cannot, in view of the considerable mobility of the dugong, be such as to entail any great burden on the limbs. Thirdly, the slender form of the appendages, their relatively small size, and finally the uninjured epidermis on the parts in question, furnish evidence against their use as supports in the water. Also in the case of the manatee, here adduced for comparison, the use of the flippers as organs of support is denied rather than maintained. Moreover, Brandt cites three authors who conclude from the manner of capturing the American manatee, that it is incapable of getting back to the water (by the aid of the limbs) if left up on the dry land. Garrod ('77, p. 139) says very decidedly that his manatee seemed *wholly unable to move either forward or backward on the land*. It made use of the limbs in sidewise movements only, at the same time twisting the body and tail. Chapman ('75, p. 461) and Murie ('80, p. 26) observed that their manatees swam quietly about at the bottom of the aquarium with the body sharply bent and the head and tail pointed downwards. Neither author says a word about their supporting themselves on their flippers, and Murie figures the animal in the posture described, with the limbs hanging down. He men-

tions their aid only in the taking of food. Brown ('78, p. 296) brings up still another argument against the possibility of a manatee being able to leave the water by aid of its flippers, namely the weakness of these organs, and his investigations into this matter also resulted negatively. Crane ('81, p. 457) makes a similar statement, basing his opinion on the generally clumsy movements of the animal when on dry land as well as on certain other considerations. Opposed to these observations are only the statements of Cunningham ('70) and Noack ('87, p. 299). The former saw a manatee support itself above the water by its flippers in order to crop the grass at the edge of the shore. This was done in such a way that one limb rested upon the shore. The latter author goes still further, for he states that the manatee is able to come out and move about on the land. "The animal (*M. senegalensis*) can actually move from place to place, though in a hobbling and unsteady manner. In so doing it supports itself on its wrists [!] and the outer edge of the forwardly directed hands, moving them alternately, and thus it progresses forward, though slowly, wriggling its body as it goes." But one may also suppose that the wrigglings of the body were the primary and locomotor movements, while the motions of the flippers might have prevented the body from falling over. In any case the position of these limbs as described is very remarkable! Noack relates that his manatee while in the water used its flippers to remove from its mouth pieces of bread that had proved too large. But as for a supporting function of these organs while in the water, Noack likewise knew of no evidence.

To sum up, it must be said, then, that scientific observation on the sea cows (for in case of the *rhytina*, also, Steller mentions that they are unable to move about on land) has hitherto produced but slight evidence that would permit us to assume so considerable a use of the flippers for body-supports as would explain the development of many anatomical details. Nevertheless, theoretically one may suppose it has only chanced that the use of the limbs as supports has been so seldom seen. But on theoretical grounds also, considerable objection may be raised against the evidence brought forward by Abel (:04, p. 166) as has already been done in the course of this discussion. We should remember

that the Sirenia have arisen from air-breathing land forms of Mammalia; further we should not forget that their terrestrial type, with its adaptations of the body to pressure and weight, is greatly modified by adaptations to an aquatic life. Thus as we trace the evolution of the Sirenia, their mechanical adaptations to terrestrial life will be less and less noticed while those for aquatic existence will increasingly force themselves upon our attention. In like manner the anatomical changes brought about by these adaptations show the same progression. Such convergent anatomical changes in animals of the same biological environment are due to the influence of identical causes. In the case of these water mammals where proof of such changes is impossible to-day, we need not always assume different causes; here the same causes were acting on different objects (*e. g.*, Cetacea and Sirenia) and therefore it is conceivable that the same causes may bring about different results.

The dugong is the host of great numbers of parasites, both external and internal. On its back, as with whales, numerous barnacles establish themselves; a few *Balanus* but mostly *Chelonobia*. Internally, at the opening of the inner nares, on the dorsal surface of the velum palatinum, but particularly in the Eustachian tubes of both sides, were found numerous leaf-shaped trematodes from 3 to 5 mm. long clinging to the membrane in a mass of an hour-glass shape. These must have been identical with the *Opisthotrema cochleare* described in 1884 by Fischer. He had received them from Semper (of Würzburg) who collected them in the Philippines from the tympanic cavity of the dugong. Leuckart also, in 1875, described a trematode from the Eustachian tube of the dugong under the name of *Monostomum dujonis*. Von Linstow ('78) considers both forms as probably identical. The favorite habitats of these trematodes give, we believe, an indication of the path by which the infection has spread in case of the dugong, namely, from the nose, to pharynx, Eustachian tubes, and tympanic cavity. To be sure, Boenninghaus (:04, p. 259) has controverted the idea that in case of the parasites of the whale's ear the path of infection has been through the pharynx and the Eustachian tubes. He himself mentions a nematode, *Pseudalium minor* Kuhn, from the ear cavity of *Phocæna*. Moreover, von

Linstow records *Strongylus arcticus* Cobb from the ear of the beluga, and *Pseudalium alatus* from the Eustachian tube and the pharynx.

In the alimentary tract of the dugong, trematodes occur in the gut and the cæcum. In the small intestine some very large worm masses were observed, whose composition could not be fully determined. In addition, a species of *Ascaris* has long been known from the digestive tract and numerous specimens of this were also found in the glands of the stomach. It is *Ascaris halicoris* Owen, also wrongly called *Ascaris dugonis* Brandt. Whether the Ascarid seen by Steller in the rhytina is identical with that found in the dugong cannot now be determined. Two endoparasites are also described from the manatee: a nematode, *Heterocheilus tunicatus* Diesing, and a trematode, *Amphistomum fabaceum* Diesing (von Linstow, '78, '89). Chapman ('75, p. 456) found the large intestine of his American manatee filled with the latter.

VETERINARY INSTITUTE OF THE
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CORRESPONDENCE

Congress of Oceanographers

Editor of the American Naturalist.

Sir:—I enclose copies of correspondence in regard to a Congress on Oceanography which is to be held in connection with the Colonial Exposition at Marseilles, France, in 1906, and in compliance with the French Ambassador's request, invite your attention to the desire of the organizers to secure the coöperation at the Congress of the associations and individuals named in the accompanying list, the *American Naturalist* being among them.

I am, sir,

Your obedient servant,

ROBERT BACON
Acting Secretary

DEPARTMENT OF STATE, WASHINGTON

October 11, 1905

Translation of Letters from French Embassy

Mr. Secretary of State.

The Minister for Foreign Affairs has sent me a letter in which Mr. Charles Rouse, Commissioner General of the Colonial Exposition to be held at Marseilles in 1906, expresses the wish to have the attention of the Government of the United States invited to the section of Oceanography of that Exposition.

In order to comply with this request, which is particularly commended by the Minister of Commerce and Industries, the French Ambassador desires to request the Federal Government kindly to invite those persons in America, who make a study of oceanography, to take the result of their studies to the Marseilles Exposition, communicating to the particular ninth section of said Exposition, the results of their research and to participate in the Congress on Oceanography which will be organized under the auspices of the section in question.

I shall thank Your Excellency for kindly informing me in order

that I may notify my Government, of the replies which the Department of State shall receive to this communication.

Accept, Mr. Secretary of State, assurances of highest esteem.

DES PORTES

EMBASSY OF THE FRENCH REPUBLIC
IN THE UNITED STATES, WASHINGTON
October 2, 1905

Mr. Secretary of State.

Under date of September 11 last, this Embassy had the honor to invite attention of the Department of State to the interest felt by the Republic in the Colonial Exposition at Marseilles, and particularly the Section of Oceanography and Sea-fishing, which is a part thereof. Referring to that communication as well as to the kind reply made by the Department of State, I take the liberty of transmitting herewith to Your Excellency a list of the associations and individuals who, in the opinion of organizers of the projected reunion, would be especially apt to contribute to its success.

I should be very grateful to Your Excellency if you would request them to signify their adhesion to this important international manifestation which promises to be of great scientific as well as practical interest.

Accept, Mr. Secretary of State, assurances of my high esteem.

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JUSSERAND

EMBASSY OF THE FRENCH REPUBLIC
IN THE UNITED STATES
NEWPORT
September 11, 1905

(No. 468 was issued Dec. 14, 1905)

